

Antiquity

VOL. XXIII No. 91

SEPTEMBER 1949

Editorial Notes

A DISCOVERY has been made in America which may be of the greatest use to archaeologists. It consists of a method of dating dead pieces of formerly living substances (such as wood and bone) by means of their radiocarbon content. The details are highly technical and beyond the comprehension of those who are not specialists. Preliminary tests on dated specimens of wood from Egypt have produced results which, as the table shows, agree very closely with the values expected; the maximum range of error is about 450 years, but the average of four samples agrees with the expected result to within about a hundred years. Further refinement of the process is expected to enable specimens as old as 20,000 or 25,000 years to be dated. That covers the whole of the Mesolithic period and takes us back to the end of the palaeolithic period and into the final phase of the Ice Age. Wood specimens are available (from peat deposits) for the whole of this period; and if the radiocarbon method, as it may be called, can be used on such, a very considerable refinement of the present British chronology will become possible. For the present methods of dating peat deposits rest ultimately on the absolute chronology built up on the basis of clay varies in Sweden, and on correlations between British and continental deposits. Other promising subjects are wooden piles from lake-dwellings, wooden handles of axes, prehistoric boats and coffins and all other organic material, such as the straw included and preserved in clay bricks and baskets, and seeds and fruit stones. We print below an extract from an article by Professors W. F. Libby, E. C. Anderson and J. R. Arnold, of the Institute for Nuclear Studies, University of Chicago, published in *SCIENCE*, March 4, 1949; Vol. 109, No. 2827, pp. 227-8; and we wish to thank the publishers for permission to do so, and Professor Libby and his colleagues for the same and for an offprint of the article. We also wish to thank Lord Cherwell for calling our attention to the discovery, which was first made known in this country by *SCIENCE TO-DAY*, Vol. 5, No. 125, 24 March 1949. We hope that it may be possible to publish in *ANTIQUITY* a full and authoritative account of the method, when further experiments now in progress have been concluded.

‘Having established the world-wide uniformity of the radiocarbon assay at the present time, it seems a logical assumption that this would have been true in ancient times. Assuming this, and using the half-life of radiocarbon, $5,720 \pm 47$ years, one can calculate the specific activity to be expected after any given time interval elapsed since the removal of any carbonaceous material from equilibrium with the life cycle. For living materials this probably coincides with the time of death; for carbonates it would correspond to the time of crystallization (assuming no further interchange with the solution or atmospheric carbon dioxide to occur). On this basis we have undertaken examination of wood samples of well established age from the ancient Egyptian tombs. Two such samples were used, one from the tomb of Sneferu at Meydum (furnished by Froelich Rainey, of the University of Pennsylvania Museum, Philadelphia) which was $4,575 \pm 75$ years old; the other from the tomb of Zoser at Sakkara (furnished by Ambrose Lansing, of the Metropolitan Museum of New York) which was $4,650 \pm 75$

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years old. The former sample is cypress wood ; the latter is acacia. John Wilson, of the Oriental Institute of the University of Chicago, has given the dates quoted, at the behest of a committee of the American Anthropological Association, consisting of Frederick Johnson, chairman, Froelich Rainey, and Donald Collier. The expected assay for 4,600-year material is easily calculated to be 7.15 ± 0.15 cpm/gm of carbon on the basis of the present assay and the half-life. Table 2 presents the data obtained on these materials.

'The data on both samples were averaged since the error in ages almost overlaps the difference, and the weighting was taken according to the error quoted in each run. The errors quoted here and in Table 1 also are standard deviations determined strictly from the statistical counting error, and, since the data agree within these errors, we believe that no other appreciable error is involved in the measurement. It is gratifying that the mean of the determination agrees with the expected value within 1 standard deviation unit. An error of 0.4 cpm/gm in the specific activity corresponds to an error of 450 years in a 4,600-year-old sample.

'On this basis we feel encouraged to proceed with further tests on younger samples of known age. This work is now in progress. It is hoped that certain unknowns can be measured in the near future. A large thermal diffusion column similar to the one used by Dr Grosse and his associates has been installed in the laboratory and a considerable increase in accuracy should result, permitting the measurement of samples as old as 20,000 to 25,000 years'.

AGE DETERMINATION ON THE EGYPTIAN SAMPLES

<i>Sample</i>	<i>Specific activity found</i> (cpm/gm of carbon)
Zoser	7.88 ± 0.74
"	7.36 ± 0.53
Sneferu	6.95 ± 0.40
"	7.42 ± 0.38
"	6.26 ± 0.41
Weighted average (both samples)	7.04 ± 0.20
Expected value	7.15 ± 0.15

With the present number of ANTIQUITY we are glad to record the accession of quite a large number of new readers, to whom we give a hearty welcome. We hope they will be satisfied with its contents. We try to act as a medium between specialists and the general reader ; but we also like to be the first to publish new discoveries. Sometimes such articles are necessarily somewhat technical, but we know (from letters received) that readers appreciate them. That is especially true where an advance in technique is described, such as that recorded in the preceding paragraphs. One can appreciate its significance without understanding the methods. Dating sites and objects is the prime objective of archaeology, and absolute dating the ultimate objective. For until an orderly sequence of events has been established there can be no history, and without an absolute chronology the relations between cultures and civilizations must remain obscure and uncertain. A period that is not dated absolutely in years is like a map of an area that has no latitude or longitude ; one cannot place it on the Earth's surface. If one could imagine such uncoordinated maps as existing separately of England and France, there would be nothing to show the existence of the English Channel, which has played so great a part in the history of both countries.

Ninety Years Ago

by JOAN EVANS

NO one, so far as I know, has as yet made any particular study of the many local learned societies that were founded up and down France in the years after the French Revolution, and of their effect upon contemporary thought. These Sociétés d'Émulation differed from the local archaeological and historical societies of England in being more all-embracing; they included in their scope pure literature, philosophy and science as well as the history and antiquities of the district, and often developed a philanthropic side as well. At the time of their foundation they were often of a free-thinking colour; but as the balance of French life came to be restored after the Revolution this element was forgotten, and the more learned priests of the neighbourhood were often included among their members.

These societies fostered a peculiar polymathic quality among those who regularly attended their frequent meetings. They were to a great extent self-supporting in the provision of papers and communications, and it would have been pure selfishness for any member with any claim to versatility to specialize too deeply. Their standards, too, were not those of the Metropolis, where a new professionalism was bringing higher and more exact criteria into every branch of knowledge and speculation. Rather, we can see in the Sociétés d'Émulation of the nineteenth century the continuance of the amateur spirit that in the eighteenth century had flourished in the aristocratic circles of Court and château: a spirit surviving in a less polished form among the lesser gentry of the provincial towns.

The Société d'Émulation de la Somme, with Abbeville as its centre, was a typical society of the kind. It had been founded in 1795 by a Monsieur de Mautort and a Monsieur Boucher de Crèvecœur who rapidly rose from Secretary to President. His son, Jacques Boucher de Crèvecœur de Perthes, began to take a leading part in its councils after 1825, and in 1827 added a small museum to its activities and revised the statutes. In 1830 he in his turn became President, and in 1832 instituted the printing of its statutes.

Jacques Boucher de Crèvecœur de Perthes may be regarded as the fine flower of members of a Société d'Émulation. He was born in 1788, at Rethel, a scion of a good country family of the lesser nobility; his father, Jacques Boucher de Crèvecœur, had for reasons of prudence dropped all claims to nobility during the Revolution, but that made no difference to his pedigree. On his maternal grandfather's side Jacques came of yet more illustrious stock, for the said grandfather was the last direct descendant in the male line of Jeanne d'Arc's first cousin, who married Colas de Perthes. When this grandfather died in 1818 Jacques Boucher de Crèvecœur assumed the name of de Perthes by royal licence in order that the name of Jeanne d'Arc's closest kin should not die out.

His family contrived to live quietly through the Revolution, and the Terror without emigrating. His father had had some acquaintance with Napoleon in his days of obscurity and at one time entertained hopes of his son's becoming one of the Emperor's pages. Instead, in 1803, Jacques Boucher de Perthes entered the Customs and Excise, and in that service travelled a good deal in a rather picaresque fashion all round the

Mediterranean. He returned to the Pas de Calais for the proposed invasion of England; held a post in Brittany for a time, and then in 1825 contrived to be nominated to a customs post at Abbeville, where his father had long been settled and where he had passed much of his boyhood. The post was unimportant; he had private means; and he was free to make the Société d'Émulation one of the chief interests of his life.

He had all the qualifications such a Society required: immense facility, and interests as varied as his ambitions. He longed to succeed as a dramatist; he wrote a vast number of less than mediocre plays, and never failed to be disappointed that he could not find anyone to produce even one of them in Paris. It remained one of the great disappointments of his life that though in 1823 he succeeded in getting his tragedy of *Persée* accepted for the Odéon, it was never performed. He had a capacity—not uncommon in men brought up on the windy oratory of the Revolution—of composing orations on general subjects: courage, the place of women in society, and so on; he rhymed easily, and had a satirical turn; he wrote "characters" in the manner of La Bruyère, and he compiled a work in five volumes *De la Création*. All these works formed the basis of discourses or readings to the Société d'Émulation; and in addition he did all the things proper to a local society, from exhibiting a giant pear to founding prizes in the local orphanage.

Such work would not have brought him fame; but besides general culture the society was interested in local antiquities, as was natural in a body that had its home in so long-inhabited an area as the valley of the Somme. M. Casimir Picard, a local doctor with a great deal less facility and a great deal more exactitude than Boucher de Perthes, had been interested in flint axes as long ago as 1830 (1). In 1836 (2) he made a communication to the Society on stone axes of a ruder type than the regularly shaped examples that were familiar to those who collected the objects found in the peat cuttings of the Somme valley. He claimed the 'haches dites ébauchées'—what we should now call palaeolithic flint implements—as being an original and early type of axe, not unfinished examples of the other more familiar kind. He had, moreover, recognized flakes as an early form of tool, and had established the relation of such flakes to a core. Picard had inherited some notion of stratification from Traulle's and Mongez' work, and on 20 November 1835, set out its importance before the Société d'Émulation and planned excavations to establish the local sequence. Unfortunately he was a busy professional man and was slow to undertake further researches; and, more unfortunately, in 1841 he died rather suddenly.

The mantle of Dr Casimir Picard fell upon the shoulders of Jacques Boucher de Perthes, who wore it with such dramatic effect that he soon forgot that it had not always been his own. He needed something fresh to divert his mind, for he had been seriously disappointed in several projects. He had planned to secure a nomination to an excise post in Paris, which was to fall in about 1843, in three years time; and to secure for himself more consideration in the city than he might otherwise have enjoyed, planned to offer his collections of antique Picardy furniture and miscellaneous objects to form a separate gallery in the Musée de Cluny which was being founded by the generosity of M. du Sommerard. By 1843 he wrote in very handsome terms to Viollet le Duc to offer his collections to the State, on condition that he was to have the arrangement of them (3). Viollet le Duc was slow to answer, but Boucher de Perthes was quick to write to every

¹ Aufrère, *Épreuves*, p. 6.

² Mem. Soc. d'Ém. d'Abbeville, 1836-7, I, 234.

³ *Sous dix Rois*, v, 400. Many of these letters are suspect, but I think this is authentic.

possible interested administrator in turn, to offer his fifteen to twenty oak cupboards and as many chests, and his medieval carvings, in return for a gallery bearing his name and a marble plaque recording it (4). In August 1843 the offer was accepted, and he was almost as much pleased as when the Odéon had accepted *Persée*. Once more, however, an endless succession of unaccountable delays prevented the scheme from reaching fruition. The authorities of the Musée de Cluny made a great fuss over the weight of his oak cupboards and the difficulty of transporting them from Abbeville to Paris. Finally, in October 1848, the gift was refused on the ground that it was not of sufficient artistic importance to be specially housed; and it did not enter into Boucher de Perthes' plans to have his collection merged in those of M. du Sommerard, even though he was offered a secondary position on the Museum staff.

In the meantime he had realized that his Parisian ambitions were unlikely to materialize, and had sought fresh laurels in the geological field. His amateur status in that field was unquestionable. In the years before 1825 when he was stationed in Brittany he had accompanied his friend Général de la Truglaye on a few geological walks; and to watch him wield a hammer and endure the consequent fusillade of granite chips had, he felt, initiated him into the science. His previous book (in five volumes) on the Creation had not taken much account of so mundane a science, but he staked his claim to Picard's subject in his *Antiquités celtiques et antédiluviennes*, of which the first volume appeared in 1847.

He begins (5) by claiming to have visited practically every excavation of an engineering kind in the neighbourhood for the last ten years; the claim may be well-founded, for he was an idle man and a good walker. He divides early history, going backwards, into five periods: Gallo-Roman, Gallo-Celtic, Celtic, Pre-Celtic, and Diluvian, and defines the scope of his book as covering the last three. He gives, without acknowledgment to Picard or any other authority, a good account of the stratification of the Somme Valley in undisturbed ground: an average of 30-40 cm. of medieval remains, then some 50 cm. of Roman, and about the same of Gallo-Roman; then some 2 metres of Gaulish; then a metre down—or about 4 metres below ground level—one is in the middle of a 'Celtic' (or as we should now say, neolithic) stratum; and below this one finds an alluvial layer of varying depth (6).

He recognizes that below the strata that contain recognizable objects of human handiwork others are reached containing bones which show that the animals they come from have been used for food, presumably by men (7). Once more following in Picard's footsteps, he distinguishes between the polished well-shaped implements of the later period (which he calls Celtic or sometimes pre-Celtic and identifies with the age of dolmens) and the ruder ones, the *haches ébauchées*, which he calls diluvian, or sometimes antediluvian. He rightly notes the absence of pottery in the stratum that produces the *haches ébauchées*, and rightly stresses that though this stratum must cover an immense length of time the axes in it show no visible development on the sites known to him. Yet he falsifies the very foundation of his argument and the whole point of the discovery by accepting the neolithic axes which his workmen had introduced into the 'antediluvian' stratum in the sandpit of Menchecourt as being indeed of the earlier period (8), and

⁴ Aufrère, *Préhistoire*, p. 74.

⁵ p. 2.

⁶ p. 165.

⁷ p. 60.

⁸ pp. 27, 183.

contemporary with the fossil remains of elephants and rhinoceros in the same stratum. (It is the greater pity because his recognition of the riverside dwelling, and the neolithic stratum on the same site are reasonable and valid). He is moreover, remarkably vague in his dating; he never commits himself to identifying the Flood that caused the stratum on which the dating of the *haches ébauchées* depends, but one suspects that he thought of it in strictly biblical terms.

His horrid little outline plates include a few scrapers and a number of flint implements of Acheulian type (9), some of which are included in his collection of 'early sculptures': flints in which he found resemblances to the human face, and to animal and plant forms. He lingers happily over his late neolithic 'Celtic' finds in bone and stone, and it is not until Chapter xvii (10) that he reaches his 'haches diluviennes', described in careful detail with considerable skill, even though no technical vocabulary for the purpose had yet been invented. (Whatever Boucher de Perthes lacked in the way of scientific acumen, he had the archaeological eye). He returns once more to the later period with his 'couteaux celtiques' and then comes to his 'couteaux diluviens' which for the most part are recognizable palaeoliths. But he shies away once more from the early stratum to continue with a chapter on dolmens full of fanciful analogies between their shapes and those of the worked stones; and then really enjoys himself in a dissertation on his imagined sculptures in flint.

It is not altogether surprising that few people took the book very seriously. His 'Celtic' (neolithic) antiquities were not unfamiliar and were acceptable enough; but his antediluvian objects were suspect from the start and had most disturbing implications of which Boucher de Perthes himself seemed to remain completely unaware.

He was once more trying to get to Paris, this time as a deputy. He stood for election to the National Assembly in the election held after the proclamation of the Republic, but failed to get in. Once more he turned to his collections. Once more he started on the familiar routine of writing to all the Parisian personalities. In September 1848 (11) Jomard and Constant Prévost came to look at the drifts where flint implements were reported to have been found and declared that they were tertiary, without giving any opinion on the implements themselves. That opinion he waited for and wrote about in vain. Archiac, in his *Histoire des progrès de la Géologie* (12), which had appeared in 1848, had discounted Boucher de Perthes' discoveries, and had damped the mild interest of the Academy of Sciences. In February 1849 Boucher wrote to M. Cordier of the Academy to ask for their final report, baiting his letter with an offer of the bone, horn and stone collections, including the antediluvian implements and sculptures, to their Museum. On 6 March, he received a report declaring that the stones were worked by man; but it was accompanied by a letter in which the authorities of the Academy declined the collections and declared their unwillingness to take any further steps in the matter or to send anyone else to inspect the objects or the sites where they had been found.

Paris was evidently closed to him, and his thoughts turned to London. On 24 March 1849 (13) he sent his book and some specimen flints to C. Roach Smith, Secretary of the British Archaeological Association, for exhibition at one of their meetings. They

⁹ See particularly plates xviii, xx, xxi, xxii and lxxvi, et seq.

¹⁰ p. 347.

¹¹ *Sous dix Rois*, vi, 197.

¹² ii, 166.

¹³ *Sous dix Rois*, vi, 215.

were duly shown to the Association on 25 April, but were not found worthy of record in the *Journal* though they are reported—probably by Thomas Wright—in the *Literary Gazette* of 28 April. The claim that they date from before the Flood is recorded, but no conclusion is drawn. Boucher de Perthes had to console himself with the qualified acceptance of his discoveries by C. Buteux in the second edition of his *Esquisse géologique du département de la Somme*; but this seemed to have only a local circulation. Dr G. A. Mantell mentioned his theories at the Oxford meeting of the Archaeological Institute in June 1851, but was more definite in refusing to accept his sculptures than in accepting his implements (14).

A pause ensued, in which Boucher de Perthes occupied himself with local activities, considerably increased in volume under the new Constitution. Only in September 1851 did he get to London (which he did not like) and see Roach Smith; but Roach Smith had collections of his own to show, and Boucher de Perthes got little satisfaction from the visit.

His great difficulty was to get any experienced observer to come and look at his pits; there was the sandpit at Menchecourt (with dishonest workmen who 'planted' flints in the places where they knew he wished to find them) and gravel pits at Saint-Acheul and Saint-Roch-lès-Amiens in which he took a lesser interest. Boucher de Perthes (who may have suspected that his own exposition of the finds was not very convincing) rightly felt that to get experts to look at his sites was the best way to convert them to his own view of their importance. In 1854 he had his first real convert. A Dr Rigollot, a good amateur geologist who had been interested in the fossil remains from Saint-Roch-lès-Amiens as long before as 1819, had always refused to take Boucher de Perthes' theories seriously. Now he made a fresh examination of the pits, was converted, and avowed it. He published a paper on the flints from Saint-Acheul (15)—a scholarly representation of Boucher de Perthes' material—and was nominated in 1855 a Correspondent de l'Académie des Inscriptions on the strength of it. He died before he could be admitted, and Boucher de Perthes tried in vain to get nominated in his stead. Once more fresh adversaries arose; the pious became anxious about the orthodoxy of antediluvian man in the valley of the Somme, and succeeded in fogging the issue; and geologists who had not visited the pits declared that the drifts were not untouched but man-made and of recent creation.

Rigollot's conversion and success spurred Boucher de Perthes to new efforts; once more he hoped to inherit the mantle of a dead prophet. In 1857 he published the second volume of his *Antiquités celtiques et antédiluviennes*: a volume rather thin in fresh content if not in bulk. He was, after all, nearing seventy. The significant passages are Chapter VI, in which he considers the relation between the 'celtic' (neolithic) and 'antediluvian' (palaeolithic) finds; he found nothing identical between them but the presumed flint sculptures of animals. In Chapter XII he discusses the fresh finds, especially at Saint-Acheul and Saint-Roch; and in Chapter XIX describes the main 'antediluvian' types—the triangular axe, which he considers the oldest, the knife with a flat butt and a lance-like point, which he considers typical of the middle stratum; and the triangular arrow, typical of the most recent. He also discusses the almond-shaped axe and the flake and scraper; the heart-shaped stone, and the disc, which he inclines to attribute to the Celtic period; the axe-hammer, the castanet-scraper (again Celtic) and other forms—which appear to be cores—for which he can find none but a symbolic use. The intervals are filled with general discussions, chiefly of the flint sculptures

¹⁴ *Arch. Journ.*, VII, 1851, p. 210.

¹⁵ *Mémoire sur les instruments en silex trouvés à Saint-Acheul. Amiens, 1855.*

which he was beginning to see everywhere. He had little chronological sense and happily includes Gaulish and Gallo-Roman sculptures in his plates ; but he sometimes had an intuition of truth, as when (in a note (16)) he asserts his belief in the gradual transformation of species under the influence of needs and environment.

Already, we may guess, he was conscious that people said he has derived his ideas of early man from Picard and Rigollot. He goes out of his way to declare that the notion that worked flints might be found in tertiary beds had first come to him in 1826 when examining a sandpit outside Abbeville (17), an assertion that can neither be proved nor disproved but cannot be unreservedly accepted.

The second volume made even less impression on the learned public than had the first. Littré, however, cited Boucher de Perthes and Rigollot in a literary article on Cave Men in the *Revue des Deux Mondes*, and on the whole came down on their side. In June 1858 the paper Boucher de Perthes read to an archaeological congress at Laon was ill received, in spite of Rigollot's publication. In August he wrote to Adrien de Longpérier, the Keeper of Antiquities at the Louvre, to suggest that a Musée Gaulois should be founded to contain the collections which he was desirous of presenting to the State (18). They are classified as antediluvian, antehistoric (a new name for ' Celtic ') Gaulish, and Merovingian. There is enough, he declares, to fill ' une vaste galerie '. The offer seems to have been politely declined : his reputation was not standing at a high level in Paris, though when in November he proposed that the Société d'Émulation should commission a local painter to create a large and partly allegorical picture of all the celebrities of Abbeville and the neighbourhood (19) he doubtless expected to find himself in the front row.

Fate, however, that had kept him waiting so long for any expert examination of his case, now relented. At the beginning of November 1858 the English geologist, Hugh Falconer, who had already pointed out the significance of worked flints and bones of cave-bear and rhinoceros being found together in Brixham Cave, happened to pass through Abbeville and saw Boucher's collections (though not his sites) and at once wrote to his friend Joseph Prestwich (20).

' He showed me " flint " hatchets which *he had dug up with his own hands* mixed indiscriminately with the molars of *E. primigenius* . . . Abbeville is an out of the way place, very little visited, and the French savants who meet him in Paris laugh at M. de Perthes and his researches. But after devoting the greater part of a day to his vast collection, I am perfectly satisfied that there is a great deal of fair presumptive evidence in favour of many of his speculations regarding the remote antiquity of these industrial objects and their association with animals now extinct . . . If, during next summer, you should happen to be paying a visit to France, let me strongly recommend you to come to Abbeville '.

Prestwich had read of worked discs from Abbeville, not in Boucher de Perthes' own book but in Buteux's *Esquisse géographique du département de la Somme* (21), and was interested. Meanwhile Lartet, whose own researches in the Dordogne district were beginning to bear fruit, had written in February 1859 to tell Boucher de Perthes that he

¹⁶ II, 273.

¹⁷ II, 362.

¹⁸ *Sous dix Rois*, VII, 295.

¹⁹ *Mém. de la Soc. d'Ém. de la Somme*, Nov. II, 58.

²⁰ Prestwich, p. 119.

²¹ Prestwich in *Proc. Roy. Soc.*, x, 50.

considered their discoveries bore each other out (22), and the academic temperature had grown a trifle warmer.

Prestwich decided not to wait for the summer, but to make an expedition to Abbeville at Easter 1859. He invited a number of his friends of the Geological Society to accompany him, and to suit them postponed the visit until the end of April. John Evans (my father) wrote to his fiancée in March: 'Think of their finding flint axes and arrowheads at Abbeville in conjunction with bones of Elephants and Rhinoceroses 40 feet below the surface in a bed of drift. In this bone cave in Devonshire (23) now being excavated by the Geological Society they say they have found flint arrowheads among the bones, and the same is reported of a cave in Sicily. I can hardly believe it (24)'.

Prestwich reached Abbeville on 27 April. He was then a man of forty-seven, a member of a firm of wine merchants in London: a man who combined great scientific gifts with so kind a heart that he would have found it hard to demolish another man's theory, but for his spontaneous need for expressing the truth that was in him. He was very tall and thin, with a surprisingly booming voice and a pleasant sense of humour. He began by paying a state visit to Boucher de Perthes, who lived in a big town house set round a courtyard entered through an iron grille, with the Société d'Émulation housed in one of the wings, and the office of the excise in the other. The central part of the house was crowded beyond belief with every imaginable antiquity: much fine pottery, many family pictures, and all the old Picard furniture that had never got to the Musée de Cluny. Boucher de Perthes, now past seventy, had 'a great deal of manner'—to use a familiar Prestwichian expression—and a very obvious wig. His evident ignorance of geology, his preoccupation with chimerical flint sculptures, and his avowed preference for literary work, were none of them calculated to inspire confidence in the London merchant who was also a scientific geologist. Yet there, among the untidy heaps of later antiquities, were undoubtedly early flint implements, together with fossil bones that Boucher de Perthes swore he had seen drawn from the same stratum.

Prestwich anxiously awaited the arrival of his friends, but the weather was bad, the crossing stormy, and of all of them only John Evans, a busy paper maker from Hertfordshire, ten years younger than himself, turned up late in the evening. 'We went straight to bed', Evans wrote, 'and soon after 7 the next morning M. Boucher de Perthes, the first discoverer of the stone axes we were in pursuit of, came to take us to some of the gravel pits from whence his collection had been derived. A M. Marotte, the Curator of the Museum [of the Société d'Émulation], accompanied us but we did not succeed in finding anything'. They adjourned to Boucher de Perthes' own collections: the old man had bought a new visitors' book for the occasion, and the English visitors duly signed on the virgin page. The visitors were greatly impressed with the collections, though some of the implements seemed to them suspect, and the sculptures accidental. The too-honest Englishmen told him as much. However, the vast majority of the axes and other weapons were obviously genuine. A message came to Prestwich from M. Pinsard, Boucher de Perthes' henchman at Amiens, that an axe had turned up in the Saint-Acheul pit and had been left *in situ* for their inspection. After a sumptuous fork-luncheon they all set off for Amiens, where they were met by Pinsard, the President of the Society of Antiquaries of Picardy, and the librarian. 'We proceeded to the pit where sure enough the edge of an axe was visible in an entirely undisturbed bed of

²² Aufrère, *Préhistoire*, p. 112.

²³ Torquay.

²⁴ Joan Evans, *Time and Chance*, p. 100.

gravel and eleven feet from the surface. We had a photographer with us to take a view of it (25) so as to corroborate our testimony and had only time to get that done and collect some 12 or 15 axes from the workmen in the Pit when we were forced to take the train again to Abbeville'. The next day they went to see the sandpit at Mencheville near Abbeville, which they found much richer in fossils if poorer in flints. They were impressed by the difference in colour between the whitish weapons from Mencheville and the dull ochreous yellow of the implements from Saint-Acheul, and by the realization of the time such a change of colour must have taken. That evening they returned to England, wholly converted.

On the evening of 5 May Prestwich entertained a number of geologists and antiquaries with his legendary sherry and a view of the implements, and they in their turn were greatly interested. Falconer generously gave up any claim to credit in the matter (26), and publication was left to Prestwich and Evans. On the 26th, Prestwich read a paper to the Royal Society (27), which, reported John Evans (28) 'would have been very good but he had only time to give an indifferent abstract of it and his voice was hardly audible in that room . . . I had written an antiquarian letter to him to incorporate in his paper, and this he dexterously managed to leave behind him. The result was I had to stand up and give up an extempore lecture . . . There were a good many geological *nobs* there, Sir C. Lyell, Murchison, Huxley, Morris, Dr Perry, Faraday, Whetstone, Babbage, etc., so I had a distinguished audience. Our assertions as to the finding of the weapons seemed to be believed'.

Belief, indeed, became possible when it was an experienced geologist, and not Boucher de Perthes, who presented the case. Prestwich described the stratification he had found at Saint-Acheul. Below the stratum of brown brick earth and flint gravel, came a second stratum of whitish marl and sand with small chalk debris and freshwater shells. Beneath this lay a bed of coarse subangular flint gravel, white with irregular ochreous and ferruginous seams, with tertiary flint pebbles and small sandstone blocks. In this layer were found teeth and bones of elephant, horse, ox and deer, and the worked flints that Boucher de Perthes calls *haches* and the workmen *langues de chat*. Prestwich—explaining as politely as he could Boucher's failure to get his discoveries accepted—ascribed it to the indifference of his illustrations and the inclusion of 'doubtful forms of an accidental kind'. John Evans, in his brief contribution at the end of the paper, classified the implements: arrowheads, knives, and pointed, truncated and almond-shaped axes. It is noteworthy that even to so learned an audience he had to explain that the flints from Saint-Acheul were not those generally called Celtic—that is neolithic—and that even the 'Celtic' were not the handiwork of the Celts. (It was not until 1865 that Sir John Lubbock invented the convenient words palaeolithic and neolithic (29).

Three days after the paper on 29 May, 1859, Prestwich returned to Abbeville with Godwin-Austen, Flower and Mylne for further verification. They opened trenches in virgin ground and themselves found fossil bones and worked flints at a considerable depth. On 23 June Boucher de Perthes gratified the Société d'Émulation with letters from

²⁵ Many years ago I remember being shown a print of this, but have failed to find it now, in spite of the search kindly made by Mr Leeds and Mr Harden among the photographs left to the Ashmolean Museum by my brother Sir Arthur Evans.

²⁶ Prestwich, p. 123.

²⁷ Reported in *Proc. Roy. Soc.*, x, 50.

²⁸ *Time and Chance*, p. 103.

²⁹ *Prehistoric Times*, I, 3.

Prestwich in which the authenticity of his discoveries of implements in the Drift was handsomely confirmed.

On 2 June John Evans read his own account of the discovery to the Society of Antiquaries (30). He had found in the volume of *Archæologia* for 1800 a paper by Frere on flint implements found at Hoxne in Suffolk in company with fossil bones; he had induced Prestwich to examine the site, and in October they employed men to dig trenches in the gravel pits and Evans discovered half an axe in a similar stratum to the axe-bearing layer at Saint-Acheul.

Meanwhile in August Sir Charles Lyell himself came to see the Abbeville pits, was convinced in his turn, and on 18 September reported on the whole question to the British Association at Aberdeen, in the presence of Prince Albert as President. The battle was won at last. It was an eventful week; the *Great Eastern* had just made its first Atlantic crossing; the men of the building trade had just struck for a nine-hours day; Brunel had died and Tennyson published the *Idylls of the King*. Yet even so the importance of the discoveries and their implications did not go unremarked. On 18 November, the *Times* published a letter from T. W. Flower reporting that he had himself found an axe *in situ* at Saint-Acheul, 18 inches inside the quarry face. On 1 December 'Senex' came into print to declare that the flints are not worked at all, but split by the action of frost and broken by the force of streams; or, if they are worked, they must be antediluvian in the strict biblical sense. It is impossible that man should have existed 14,000 years ago as Horner and Darwin pretend. John Evans answered him two days later, laughing gently at his perplexities and stressing the importance of the Hoxne finds of 1797. On 5 December Senex replied much out of temper and asking what is drift? But he and his like could do nothing against proven fact; and it was the proof of the antiquity of man in 1858 which made the theories of Darwin's *Origin of Species*, published in 1859, directly applicable to *Homo sapiens*.

Boucher de Perthes was thankful for his English friends' backing, but more anxious for his own reputation than for the scientific implications of his discoveries. We may guess that his friends in France had not wholly forgotten Picard. In a letter to Prestwich of 31 May 1859, Boucher claims to have sent axes to the British Archaeological Association 'vers 1842': in fact it was not until seven years later. By the autumn, however, his fame was assured. Prestwich had addressed a letter to the French Academy of Sciences on the importance of Boucher de Perthes' discoveries. In consequence Albert Gaudry went and investigated them and confirmed their validity, and was followed by a procession of those who formerly had not thought it worth their while to come. On 4 November 1859, Hugh Falconer wrote to Prestwich (31):

'I have a charming letter from M. Boucher de Perthes, full of gratitude to *Perfide Albion* for helping him to assured immortality, and giving him a lift when his countrymen of the Institute left him in the gutter. He radiates a benignant smile from his lofty pinnacle on you and me—surprised that the treacherous Leopard should have behaved so well'.

By December, Boucher de Perthes was once more offering his collections, this time to the Museum of Natural History (32), which at least accepted a number of specimens. On 7 June 1860, he made a grand address to the Société d'Émulation (now Impériale) (33) to claim that it was now nearly a quarter of a century since he had revealed to its members

³⁰ See *Archæologia* xxxviii, 280; *Proc. Soc. Ant.* iv, 329.

³¹ Prestwich, p. 141.

³² *Soux six Rois*, vii, 381; vii, 436.

³³ *Mémoires*, 1860, p. 471.

the extreme antiquity of man and his probable contemporaneity with huge antediluvian animals. By this time he dated his first interest in the subject to 1805, when he had visited the Grotte de Roland near Marseilles and had found bones; and he dated his collections from 1836, with a pleasing vagueness as to the epoch of the objects they included.

The sunshine of his belated glory shone yet more brilliantly in 1862, when he had the chance of speaking to the Emperor of his discoveries, but was clouded in 1863 when his discovery of a human jaw at Moulin Quignon was not accepted by his English friends (34) though welcomed by the Frenchmen who had before been sceptical about his discoveries. By this time the old man was beginning to fail, and the English sceptics hardly like to press too hard their conviction that the Moulin Quignon jaw was modern and the implements found with it fakes; and that the whole find had been 'planted' by the workmen for Boucher de Perthes to discover.

The best things in his collections were now enshrined in the Musée Impérial at Saint Germain, though much still remained in the house at Abbeville, which he had bequeathed to the city to form a permanent museum. The chief occupation of his declining years was the preparation of a work in eight volumes entitled *Sous dix Rois*. It included 1355 letters in which the story of his life was told. In them one may often suspect that the pen of the novelist and dramatist has improved upon the original draft, especially in the passages in which a pre-Picard interest in prehistory is alleged. A letter from Morlaix of August 1823 (35), for example, drags in the question of 'ces haches dites celtiques' and the fact that he has already found several near Abbeville (36). The book was hardly out before Boucher de Perthes died on 2 August 1868, aged eighty. His fame was safe. The *Petit Larousse* does not include Picard, Rigollot, Falconer, Prestwich or Evans, but it duly records 'Boucher de Perthes, Jacques . . . créateur de la science de la préhistoire sur l'homme'.

It was in August 1900 that my father took me to Saint-Acheul. He had often stopped at Amiens to visit the pits on his way to Paris in the years since that momentous visit in 1859; he still had friends of long standing among the workmen. They brought out the season's spoils of yellowed flint, and my father bought three or four implements for me. The workmen in their deep-belted baggy corduroy trousers looked on with the benevolent approval that any Frenchman gives to a family scene, as my father stroked and balanced the implements in his experienced hands and handed them over to my childish ones. Old Monsieur Boucher de Perthes and his flints, and my father and Prestwich and all their excitements and enthusiasms, were ancient history to them, but they were pleased and amused that the old Englishman, whom they liked for his old-fashioned politeness, should turn up with a child of seven. I doubt if any of them—my father included—remembered on that sunny August morning that in that gravel pit a new science of prehistory had had its beginnings. I at least had the sense of being initiated into the mystery of my father's lore; I treasure those flint implements to this day.

³⁴ See *Time and Chance*, p. 116.

³⁵ IV, 309.

³⁶ Another rather suspect passage occurs in a letter to Becquerel, 14 March 1838, v, 185. See also v, 548 and 558.

NINETY YEARS AGO

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Human Evolution: a review

by E. A. HOOTON*

FOR more than half a century Sir Arthur Keith has been one of the world's leading authorities on the physical evolution of man. As a young man, Sir Arthur spent some years of medical practice in Borneo and became interested in the comparative anatomy of the anthropoid apes and the relation of these higher primates to man. By dissections and other anatomical studies he accumulated the largest body of accurate scientific information on this subject available to students before Professor Adolph H. Schultz began his researches in this same field in the nineteen twenties. In 1901 Sir Arthur published his *Human Embryology and Morphology*, which is one of the few technical treatises on man's embryology that is anthropological and primatological in its orientation. From 1908 onward Keith concerned himself with the problem of the transformation of the body and brain of the ape precursor into that of a human being. This subject led him into intensive studies of the skeletal remains of fossil men and apes, culminating in his great works, *The Antiquity of Man* (1925) and *New Discoveries Relating to the Antiquity of Man* (1931).

During the long years in which he was thus engaged, Sir Arthur was also considering the evolution and differentiation of the modern races of man, not only from the biological and anatomical viewpoints, but also on the bases of history, social anthropology, and psychology. Recently, as he approaches the end of his fruitful scientific career, this great anthropologist has produced two volumes dealing with human evolution in its entirety—a preliminary book *Essays on Human Evolution* (1946), and the present work.

The study of anthropology has diversified into so many specialities that it is now well-nigh impossible for any single individual to keep abreast of all of the advances in the numerous fields: physical anthropology, archaeology, technology, social anthropology, psychology, linguistics, etc. The specialists in these various subdivisions, all of whom may call themselves anthropologists, often speak in tongues that are hardly mutually intelligible; they often represent opposing schools of thought and interpretation (e.g. heredity versus environment). The anthropologists who study culture sometimes pay very little attention to human biology and *vice versa*. Unfortunate antagonisms have developed between these diverse classes of students of man. It is hardly possible, then, for the anthropologist whose orientation is primarily biological to produce a general work on human evolution that will be received with approbation by cultural anthropologists. Sir Arthur Keith undoubtedly recognized these difficulties and hazards before he committed himself to such an undertaking. However, he has always been a courageous and independent scientist, ready to express his opinions and convictions whenever he considers them to be substantiated by valid evidence, quite irrespective of their conformity, or lack of it, to current scientific opinion and popular prejudice.

In *A New Theory of Human Evolution*, the author begins by summarizing his 'new or group theory'. His thesis is that evolution has taken place mainly through the

* *A New Theory of Human Evolution*. By Sir Arthur Keith. London, Watts & Co., 1948, pp. VII, 451. 21s.

struggle for survival of small, local, inbreeding groups ('evolutionary units'), which were in existence from the earliest times when the development of the human brain made speech possible and right down to the dawn of civilization. Tribes (large, isolated, inbreeding evolutionary units) represent a second stage of the process, and relatively late in human development these tribal units are converted into nations. So far, there is nothing in Sir Arthur Keith's presentation that can be called 'subversive'. However, he holds that two opposing aspects of man's nature 'must be balanced to secure continuous, progressive evolutionary changes'. These are the 'code of amity' which favours the growth and exercise of all of the generally recognized Christian virtues, and the 'code of enmity' which encourages antagonism, suspicion, distrust and contempt of competing evolutionary groups and hostility toward them. It is this stress upon the continuing importance in human evolution of the qualities that are 'condemned by all civilized minds' ('emulation, envy, the competitive spirit, deceit, hate, anger, ferocity, and enmity') that is unlikely to commend Sir Arthur Keith's 'new theory of human evolution' to idealistic and internationally-minded scientists and laymen.

In detail and successively, Sir Arthur documents his theory with the citation of opinions of authorities on the nature of original evolutionary units, with synopses of the evidence of the existence of such units among the Primates and in the various human groups, with a discussion of the importance of the ownership of territory as a factor in human evolution, of group spirit, patriotism, cooperation and competition, mental bias, resentment and revenge, and the search for status (drive for superiority). He proceeds to discuss human nature as it is, rather than as it ought to be, and the effects upon evolution of leadership, loyalty, and morality.

The next portion of the book deals with the machinery of evolution; the genes, the rôles of isolation and inbreeding (endogamy, exogamy, monogamy); sex differentiation and the sex hormones; sexual and racial selection, and foetalization. In this section Sir Arthur handles subjects in which he is a recognized authority. There follows a most interesting and important exposition of the spread of the anthropoidal ancestors of mankind and of the differentiation of modern man into five major physical groupings, based upon development in five major areas of the Old World. Keith postulates Africa as the original cradle of humanity and assumes that the 'Dartians' (the man-apes or ape men recently discovered in fossil deposits in South Africa) were responsible for carrying abroad the genes that gave rise to the various human types subsequently differentiated in the several major areas. He then discusses the relation of types of fossil men to modern races. He believes that the separation of modern races is very ancient—traceable to the beginning of the Pleistocene—and that each evolved in its own area. Many resemblances between extant human races are due to 'parallel evolution' or 'convergence'. The issues dealt with in this part of the book are highly debatable. Perhaps most interesting to the physical anthropologist is Sir Arthur's abandonment of his position as the champion of the early Pleistocene existence of anatomically modern man (*Homo sapiens*) and his acceptance of the belief held by Hrdlicka, Weidenreich, and others that modern man is a direct descendant of such apelike Pleistocene human forms as Neanderthal man. This apostasy comes at a time when the discovery of Swanscombe man and the very recent find of Fontéchevade man (the latter subsequent to Sir Arthur's book) seem finally to confirm the correctness of his earlier view, although the researches of Ashley-Montagu and Oakley last summer have probably disposed of the claims of Galley Hill man to high antiquity, in agreement with Keith's latest opinion.

The next sections of the book discuss the development of civilization; hunting, agriculture, village communities and the transformation of the latter into city units

and city states in Mesopotamia and Egypt. Then comes an exposition of the author's ideas on the evolution of European nationalities from local evolutionary units, exemplified by Scotland, together with a discussion of the classification of the races and peoples of Europe. Here is a most interesting heresy on the part of Keith. He proposes to revert to the earlier use of the term 'race' as applied to small, local, inbreeding groups and to abandon the current anthropological practice of applying the term only to a major physical grouping of mankind, based upon the possession of a distinctive combination of inherited anatomical features. Sir Arthur regards the smaller local groups as essentially the 'race-makers' and proposes to use the term 'variety' for the larger differentiated physical group. All the world except the anthropologists use the word 'race' to apply to groups that are territorial, linguistic, political, and cultural, and Keith's proposal is a reversion to a substantially similar, but better defined usage, of the term. I suspect that this suggestion will not be acclaimed among anthropologists.

The final essays of the book deal with nationalism, racialism in South Africa, national self-determination as illustrated by the case of the Irish Free State, the Jews 'as a nation and as a race', nation-building in the United States, the rise of nations in the British Dominions, and, finally, the somewhat dim prospect of an evolutionary future of man in which competition between nations will be suppressed by mutual agreement and peace will reign.

Nearly all of these subjects are very hot potatoes. Sir Arthur grasps them fearlessly and firmly; he certainly does not juggle with them, but I doubt that his handling and his serving out of portions will satisfy the palates and appetites of all physical anthropologists and of more than a few cultural anthropologists and practitioners of the 'social sciences' in general. There is a grim realism (mistakenly condemned as 'pessimism') which recognizes that men are still ruthlessly competitive brutes, and that associations of human beings, such as nations, cannot easily be converted to altruistic attitudes and humanitarian endeavours, in spite of our individual ideals and aspirations. Any one who presents these unpleasant views is sure to be accorded the traditional reception of the bearer of bad tidings.

Sir Arthur Keith himself is one of the most kindly and tolerant humanitarians whom this reviewer has ever been privileged to know. He is a great biological anthropologist and his mature reflections on human evolution should be read with care and respect by all persons interested in the present evolutionary state of man and in his future prospects. They should not be dismissed with scorn and contempt because they are out of line with roseate vistas of an era of universal fellowship of man which we may like to think is just around the corner. We are likely to turn a great many corners and be confronted with many ugly and depressing prospects before we arrive at this golden goal (if we ever do).

There are many points of fact and theory in *A New Theory of Human Evolution* that this reviewer would like to discuss and debate with Sir Arthur and with the readers of ANTIQUITY. Space forbids, and, in any event, I do not believe that the proper function of a reviewer is to inject his own opinions and theories into what should be an unbiased summary of another man's work. I will say merely that I think no anthropologist should neglect to read this book and I hope that each of its readers will display the tolerance of dissenting opinion and the readiness to recognize the errors of his own positions, and to revise them, that has always characterized Sir Arthur Keith.

The Origin of Neolithic Culture in Northern Europe

by V. G. CHILDE

TILL 1948 the coherent record of farming in Northern Europe began with the neolithic culture represented in the Danish *dysser* ('dolmens') and most readily defined by the funnel-necked beakers, collared flasks and 'amphorae' found therein. As early as 1910 Gustav Kossinna (1) had remarked that these distinctive ceramic types, and accordingly the culture they defined, were not confined to the West Baltic coastlands, but recurred in the valleys of the Upper Vistula and Oder to the east, to the south as far as the Upper Elbe and in northwest Germany and Holland too. He saw in this distribution evidence for the first expansion of *Urindogermanen* from their cradle in the Cimbrian peninsula. In the sequel Åberg (2) filled in the documentation of this expansion with fresh spots on the distribution map and Kossinna himself (3) distinguished typologically four main provinces or geographical groups—the Northern, Eastern, Southern and Western. Finally Jazdrzewski (4) gave a standard account of the whole content of what had come to be called *Kultura puharów lejkwatych*, *Trichterbecherkultur*, or *Tragtbaegerkulturen*. As 'Funnel-necked-beaker culture' is a clumsy expression and English terminology is already overloaded with 'beakers', I shall use the term 'First Northern'.

The origin of this vigorous and expansive group of cultivators and herdsmen has always been an enigma. Not even Kossinna imagined that the savages of the Ertebølle shell-mounds spontaneously began cultivating cereals and breeding sheep in Denmark. As *dysser* were regarded as megalithic tombs and as megaliths are Atlantic phenomena, he supposed that the bases of the neolithic economy were introduced from the West together with the 'megalithic idea'. But the First Northern Farmers of the South and East groups did not build megalithic tombs. Moreover, in the last ten years an extension of the North group across southern Sweden as far as Södermannland (5) has come to light, and these farmers too, though they used collared flasks and funnel-necked beakers, built no dolmens either. In any case there was nothing Western about the pottery from the Danish *dysser*, and Western types of arrow-head are conspicuously rare in Denmark (6).

Becker's recent study (7) has put the whole question in a new light. It began with a re-examination of pottery recovered from Danish peat-bogs. It was a practice of the

¹ 'Ursprung und Verbreitung der Urfinnen und der Urindogermanen', *Mannus*, I.

² *Das nordische Kulturgebiet in Mitteleuropa*, 1918.

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⁵ Bagge and Kjellmark, *Stenåldersboplatserna vid Siretorp i Blekinge*, Stockholm, 1939; Florin, 'Vrå Kulturen', *Kulturhistoriska Studier tillägnade Nils Åberg*, Stockholm, 1938.

⁶ Clark, *Man*.

⁷ C. J. Becker, 'Mosefundne Lekar fra yngre Stenalder: Studier over Tragtbaegerkulturen i Danmark', *Aarbøger*, 1948 (also published separately), 318 pp. Danish text, xviii pp. English summary, xxviii plates, København, 1948.

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Early Neolithic Danes to deposit in peat-bogs complete pots and other articles, presumably as votive offerings. (Incidentally other groups of First Northern farmers observed the same custom and its roots may be traced back to the early Mesolithic and Upper Palaeolithic offerings cast into meres at Meiendorf and Stellmoor (8)). As a consequence Danish museums contain large numbers of such vessels, and the scientific examination of Aamose has added new vases, some pollen-dated. These made it desirable to re-examine the whole assemblage and compare it with vessels from tombs and settlements that can be placed in the recognized sequence of neolithic cultures.

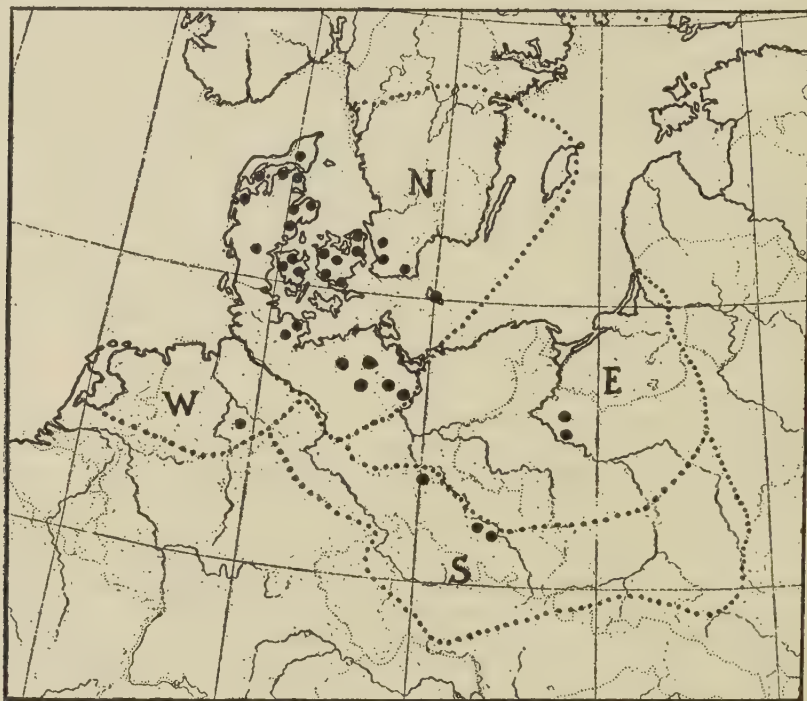


FIG. 1. DISTRIBUTION OF FIRST NORTHERN VASES OF TYPES A AND B
The approximate boundaries of the four provinces in Phase C are indicated by dotted lines

Becker has been able to work out a typological series of four stages, best exemplified in the funnel-necked beakers but supported by the associated types. The general direction of the evolution is adequately established by the associations of the later stages. Beakers of stage D occur in early Middle Neolithic (Passage Grave) sites like Trøldbjerg, those of stage C in *dysser* and contemporary earth-graves. Stages B and A should therefore be older than the first *dysser*. B types are represented in the settlement at Havnelev in Zealand, in a celebrated shaft-grave at Virring in Jutland—which Brøndsted had claimed ten years ago as 'pre-Dolmen'—and in certain shell-mounds.

⁸ Rust, *Die altsteinzeitliche Rentierjägerlager Meiendorf*.

⁹ *Wiadomosc Archeolog.*, xv, 1938, 1-105.

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Becker is then left with a small group of curious flat-bottomed vases—eleven beakers and two amphorae—which, as they already contain casts of grain, must rank as the products of the earliest farmers to clear land in Denmark. No evidence is published to prove the priority of A over B types—indeed in the Solager shell-mound both occur together in the same layer (II) though below C types—but pollen analysis may establish it. In any case A and B are certainly pre-megalithic. The A types are even less Western than the C vases

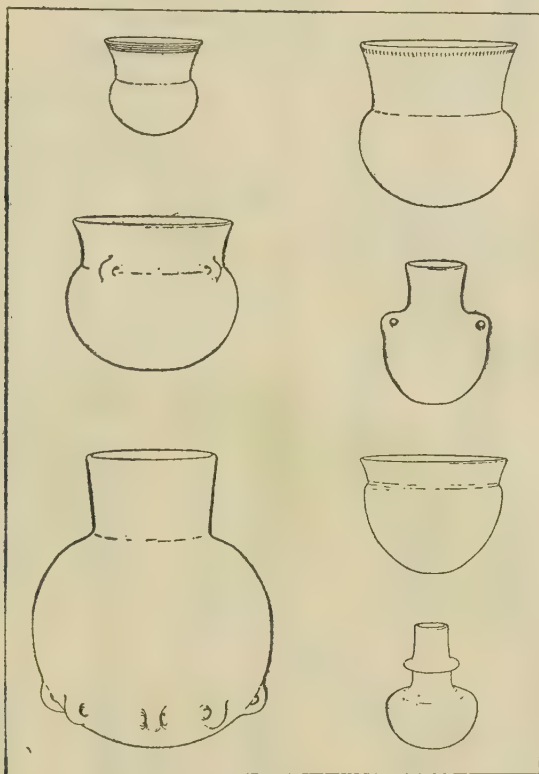


FIG. 2. TYPE OF FIRST NORTHERN POTTERY FROM DENMARK
after Becker

from *dysser*. Though Becker is inclined to admit Western, 'Michelsberg' influence to account for the rounded bases of B types, he feels obliged to look elsewhere for the origin of the first Danish farmers.

Applying this result to other regions, Becker finds B types in southern Sweden and what he calls A/B types of beaker and amphora in northern Germany and between the Oder and the Vistula as far south as Wroclaw. The latter already occupy the East and South provinces of the First Northern culture as defined by Jazdrzewski and Kossinna. They are never associated with the assemblages described there by these authors, but

are at least typologically anterior thereto. They represent prototypes from which the mature East and South groups might have developed locally just as the North group did in Denmark. A new discovery described by Becker shows this local development

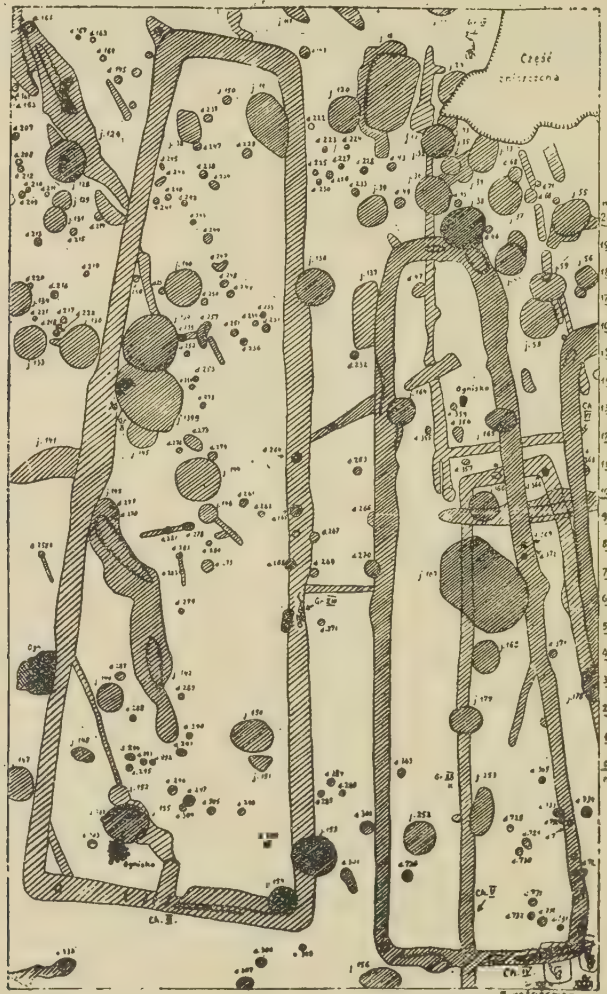


FIG. 3. PLANS OF HOUSES AT BRZESC KUJAWSKI, POLAND

was more than a mere possibility, for it establishes the true chronological relation between the East group and the North group.

An earth-grave at Salten near Silkeborg, Jutland, assignable by its form and by the arrow-heads, axes and beads it contained to stage c, contained also a small embossed disc of copper (according to a footnote analysis implies that it was made of smelted copper).

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This was certainly not made in Denmark but imported. An almost exact parallel comes from a grave at Brzesc Kujawski (between Warszawa and Poznan) which can in turn be shown to be contemporary with the mature East group of funnel-necked beakers. The

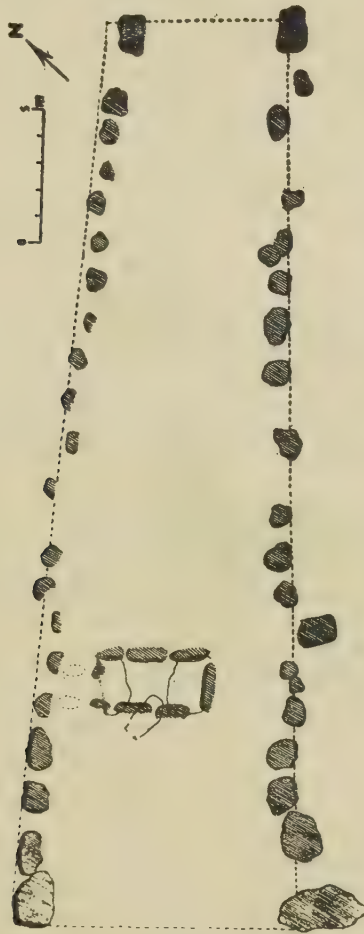


FIG. 4. PLAN OF LONG BARROW
AT DWASIEDEN, RÜGEN

latter therefore cannot be derived from the Northern group by a slow migration ; an actual interchange of trade goods shows it rather to be contemporary with the Northern group in its phase c. An equal parallelism in time may be assumed between the earlier A-B phase of both groups. Indeed these early continental vases may have been made by the ancestors of the first farmers who appear as immigrants in Denmark, and the origin of the First Northern culture must be examined from this new stand-point.

The pot forms still fail to indicate kinship with any known neolithic group. So Becker looks to some undefined cradle in the east whence the whole culture might have come fully formed. But his discussion of the Danish *dysser* helps to clear the way for a more economical hypothesis. Using the results of Thorvaldsen's article on 'Grave-finds from the Dolmen period' (10) Becker in effect shows that at the beginning of phase c the First Northern Farmers in Denmark did not bury their dead in collective megalithic tombs. A third of the tomb-groups of this phase in fact come from earth-graves containing a single extended skeleton laid on the ground within a ring of stones, perhaps protected by wooden planks and probably always covered by a low but elongated barrow (that at Salten was 19.5 m. long, the grave lying 5 m. from the east end). The earliest *dysser* themselves were completely closed stone boxes, built indeed of huge erratics but too small to be family vaults. Of 14 measurable early examples ten were less than 2 m. long inside. Eight stood in a long rectangular mound defined by a peristalith of big boulders but never near the edge so as to be accessible from outside. Functionally such *dysser* are quite different from the classical megalithic tombs of Atlantic Europe. They are single graves, designed to hold one extended skeleton. Now individual burial in the extended attitude, sometimes in cists of thin slabs, was the normal burial rite also in the East and South groups. On the moraines of Denmark erratics provide the only stone readily accessible for building, but it cannot easily be split up into slabs. The *dyss* is thus a local version, conditioned by available material, of the type of sepulchre common to all First Northern groups. Extended burial is in fact one of the distinctive traits of that culture (11).

But burial in the extended attitude, contrasting with the crouched or flexed position approved by most neolithic societies, had been practised already in mesolithic times in the North (12). Now other traits link the First Northern barbarians with the savages who had hunted and fished on the forested plain of Northern Europe before them—a preference for flint as the material for axes, transverse arrow-heads, battle-axes (for the polygonal stone battle-axes, current among all groups by phase c, must be derived in the last resort from antler axes such as were used by Ertebølle people in Denmark and still buried in graves at Brzesc Kujawski), an affection for amber, votive offerings in bogs. Hence even in 1946 (13) I had hinted that the First Northern culture might be due to the acculturation of mesolithic survivors. But such acculturation could not have taken place in Denmark. There the First Northern Farmers were the immigrants, sharply contrasted to the hunter-fishers who were still adding to the celebrated Ertebølle shell-mounds. And no other immigrants have been detected (the 'corded ware' of Virring and Vrå turns out to be only a variant of the local First Northern pottery in its phase b).

But between the Oder and the Vistula Danubian peasants had spread beyond the löss onto the once glaciated plain and had even reached the Baltic coasts. Precisely here we now find an early stage of the First Northern culture represented. Here then—presuming a survival of mesolithic Forest folk during Atlantic times—hunter-fishers could have acquired grains and sheep and learned to make pots from the newly arrived Danubian farmers. Of course they would not imitate their masters' gourd-shaped pots, but would

¹⁰ 'Dyssetidens Gravfund i Danmark', *Aarbøger*, 1941.

¹¹ Jazdrzewski, *Kultura Pucharów*, 336.

¹² Fresh well-attested Ertebølle burials are reported in *Fra Nationalmuseums Arbejdsmark*, 1945, p. 6, and Mathiassen, 'En Boplads fra ældre Stenalder ved Vedbaek Bolbaner', *Søllerød Bogen*, 1946.

¹³ *Dawn of European Civilization*, 4th edit., 176.

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reproduce in the new medium the old skin and bladder vessels, and perhaps they would further degrade the predatory Danubian rural economy. So, practising the most wasteful form of *jhuming* cultivation, they would spread quite fast and soon reach Denmark where the peat bogs reflect just such a transitory occupation of the land—clearance by fire of a patch of forest which was then allowed to regenerate (14).

As a topical appendix I might mention a curious circumstance. The Danubian peasants lived in very long houses, some rectangular others trapezoid in plan. Some at Brzesc Kujawski (FIG. 3) were as much as 32 m. long, 10 m. wide at the south end but only 5 m. at the inner extremity. Now some First Northern farmers in the East and South groups laid out the long barrows over their graves on a very similar plan. A couple of Kujavish graves in Poznań (15) of phase C were 80 m. long and almost triangular in plan. Some tombs in Rügen (FIG. 4), of rather uncertain age, are strictly trapeze-shaped. That at Dwasieden (16) was 34 m. long, 6 m. wide at one end and 11 m. at the other and contained a small chamber near, but not at, the wide end. In the South group the Pohlberg near Latdorf was again a trapeze shaped barrow, 25 m. long covering a cist only 1.4 m. in length. It is tempting to see in this curious plan an attempt to make the house of the dead approximate to habitations such as are illustrated at Brzesc Kujawski. Sprockhoff (17) indeed has pointed out that in its original form a North German long barrow (*Langdolmen*) would look very like a house with the wall timbers represented by stones and the thatched-gabled roof by turf. Can similar ideas have influenced the builders of our long barrows? In any case, Prof. Glob, in describing the early Neolithic village of Barkaer in Jutland (18), has just compared the long rows of contiguous rectangular houses there exposed to the Danish *langdysser*—long rectangular stone settings enclosing one or more megalithic cists ('dolmens'); he similarly compares the *runddysser*—one 'dolmen' surrounded by a circular peristalith—to the round houses found, side by side with the long Barkaer type, by Winther at the Middle Neolithic site of Trøldbjerg on Langeland.

NOTE. Owing to the author's absence abroad he has not been able to correct proofs, and one or two references remain incomplete.—EDITOR.

¹⁴ Iversen, 'Landnam i Danmarks Stenalder', *Dansk Geolog. Undersøgelse*, II R., no. 66, 1941; summarized in *ANTIQUITY*, XIX, 1945, 61.

¹⁵ Jazdrzewski, op. cit. 383 and fig. 980.

¹⁶ Sprockhoff *Die nordische Megalithkultur*, 1938, 31.

¹⁷ op. cit. 10, 'Das Grab erschiene wie ein niedriges Haus mit flachem Firstdach'.

¹⁸ P. V. Glob, 'Barkaer', *Fra Nationalmuseums Arbejdsmark*, 194-.

Peru before Pizarro

by G. H. S. BUSHNELL

TWO notable events in Peruvian archaeology, which are of wide general interest, have recently taken place. One was the discovery of pre-ceramic horizons on the Peruvian coast, and the other was a conference held in New York in July 1947, at which several acknowledged experts felt that the time had come to explain the known facts in terms of a general scheme of development, and attempted to do so independently with strikingly similar results. The papers read at the conference on this and other matters have recently been published (1), and they include the fullest summary so far available of the preceramic discoveries.

Mr Junius Bird has long been known for his work on the prehistory of unpromising and difficult regions in South America. He has studied successions in Tierra del Fuego and on the southern end of the Chilean mainland, which contain stone and bone artifacts but no pottery, from which he estimates, by such methods as the rate of rise of land and of accumulation of deposits, that human occupation began about 5000 years ago, i.e. at the beginning of the 3rd millennium B.C. (2). His methods of course involve some very large assumptions, but the results are reasonable when considered in relation to the usual estimate of about 10,000 years for Folsom man. He has also discovered non-pottery and non-agricultural horizons in the middens of the north part of the coast of Chile, but these may not be of any great age and their poverty may be due to the inhospitable nature of the region (3).

His latest work in Peru has been done in the Chicama and Virú valleys on the north coast, but publication is so far limited to what he calls a tentative résumé because the collections have not yet arrived in the United States for detailed study. The most notable result was the discovery of some 40 feet of pre-ceramic midden deposit in a mound called the Huaca Prieta in the Chicama Valley, containing cultivated cotton, gourds, squash, peppers and beans, besides remains of wild food plants and a variety of animal foods, chiefly derived from the sea. An important point is that there is no maize, a blow to the theory that the development of agriculture and civilization in America rests on this plant alone. Cotton textiles, mainly twined but some woven, were found, also fish nets and twined baskets. Bark cloth was made. Gourds were used as containers and floats for fish nets. Coarse stone flakes, cores, hammerstones and net weights were found, but there was no pressure flaking of stone. Subterranean houses of one or two rooms, which occur in the upper part of the deposit, apparently belong to the period. They lack hearths and cooking was done with firestones. The author acutely calls attention

¹ *A Reappraisal of Peruvian Archaeology*. Assembled by W. C. Bennett, pp. ix, 128, 72 figs., 6 tables. *Memoirs of the Society for American Research*, no. 4, 1948. (Supplement to *American Antiquity*, vol. XIII, no. 4, Part 2, April, 1948), Menasha, Wis.

² Bird, J. B., in *Handbook of S. American Indians*, vol. I, pp. 17 ff (Bureau of American Ethnology, Bull. 143).

³ Bird, J. B., *ibid.*, vol. II, p. 587 ff.

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to what our view of the culture of these people would have been, if nothing but their stonework had survived.

A junction between this and a pottery-bearing culture was obtained not in but near this mound, and again near Gualiapa in the Virú valley, though little perishable material was preserved at the latter site. The first pottery at both places was a generally similar plain ware, and it appears that the succession near Huaca Prieta passes up conformably, in geological language, from this to the more familiar Cupisnique or Coastal Chavin horizon at the base of the previously known succession.

In the Virú valley Bird found a soil profile which seems to show continuous aggradation from the deposition of coarse gravels associated with the recession of the ice in the Andes until quite recent times, when erosion set in. Here Cupisnique sherds were found 6 feet above the base of a 21-foot section, whence the author assumes that the events between the end of the last glacial period and Cupisnique times, including the pre-ceramic occupation, must have lasted only about two-fifths of the time occupied by the previously known pottery-making periods. By methods similar to those he employed further south he estimates that agriculture was established in this region about 5000 years ago, at roughly the same time, incidentally, as he believes human occupation to have begun in southern Chile. This, however, puts the introduction of Cupisnique pottery at about 1800 B.C., instead of about the beginning of the Christian era, as most other people guess. Kroeber, in summing up the results of the whole conference, notes the discrepancy, but believes that the two estimates at least provide us with outside limits.

In addition to the pre-ceramic agricultural remains, Bird and others have found several workshop sites, with abundant pressure-flaked tools but no sherds or camp refuse, in the same neighbourhood. The pressure-flaking shows that these belong to a different culture, which he tentatively ascribes to an older nomadic hunting people, but it seems that a good deal more work will be necessary before this is proved, and experience in Ecuador suggests to the writer that later pottery-making peoples who used stone implements may perhaps have prepared them on such sites. It seems also that Bird may have difficulty in fitting a pre-agricultural period into his 6 feet of post-glacial but pre-ceramic deposit in the Virú valley, so if such a period exists in this region it may indicate, as we suspect on other grounds, that this section is not as complete as it appears to be, which would invalidate his age estimates. Further developments in this connexion will be awaited with great interest.

Turning to the other main theme of the Conference, which we may call, in plain terms, giving meaning to Peruvian archaeology, there are contributions from W. C. Bennett, Gordon Willey and Duncan Strong, all of whom have been active in the field in Peru and have an intimate knowledge of artifacts and styles, and J. H. Steward, whose work of editing the great 'Handbook of South American Indians' (4) must have given him an unrivalled grasp of what is known of economics and social, religious and political organization. There are minor differences in their explanations, particularly in terminology, but the measure of agreement is striking, and an article giving a very similar interpretation for Middle America is contributed by a distinguished Mexican archaeologist, Pedro Armillas.

The pre-agricultural period, whose existence in Peru is still speculative, and the early farmers have already been discussed and do not call for further comment here. Then comes the Chavin culture, called Cupisnique on the north coast, to which the

⁴ Four volumes of the Handbook have been published so far, see note 2.

different authors apply the terms Inter-Areal Developmental, Cultists and Formative (early), in which the communities centred in temples, the art showed a strong religious bias with general similarities over a wide area, and there were rapid technological developments. It is inferred that there were widespread peaceful contacts between communities which were predominantly theocratic.

Next come a variety of cultures, known to specialists as Salinar, White-on-Red, Cavernas, Interlocking, Gallinazo and others, which the authors class as Regional Developmental, Experimenters and Formative (later). Technical developments in agriculture and manufactures approach their limits, and the rise of local styles foreshadows the intense regionalism which was shortly to grow up.

Florescent, Regional Florescent and Master Craftsmen are the terms applied to the succeeding stage, which includes the well known cultures of Mochica, Nazca, Recuay and part of Tiahuanaco. The word 'Florescent' perhaps requires some explanation; it is meant to describe a period in which a number of regions, each sharply differentiated from its neighbours but standardized within itself, reached their highest artistic level. Mochica pottery, with its vast variety of forms and painted scenes, all of them easily recognized as Mochica, provides an admirable and well-known example. The great temple mounds and the ritual scenes on the pottery show that the theocratic state was still dominant, but side by side with this we find representations of warriors, captives and trophy heads, which indicate the rise of a military hierarchy, possibly connected with the increase of population beyond the optimum in each region.

After this, some writers recognize a somewhat obscure epoch of Expansion or Fusion perhaps marked by military conquest, certainly by unrest, associated with the widespread development in the coast of Tiahuanaco-derived, Black-White-Red, and less known pottery styles, which obliterate the strongly regional styles of the preceding stage. Finally all agree in postulating a period of Empire and Conquest, marked first by the Chimu Kingdom on the coast and then by the well-known Inca Empire to which it succumbed, whose rise is a matter of history. These are associated with the construction of urban centres and fortifications, which rival or surpass the temple mounds.

It is not necessary to notice in detail the parallel interpretation of events in Middle America given by Sr. Armillas, but it is worth noting that he minimizes the effect of movements of population on the development of the militaristic stage, ascribing it rather to internal economic crises. Kroeber in his summing up calls attention to a point of great importance, namely the degree of contact between Peru and Middle America. Are the apparently parallel developments in the two areas real, or the result of wishful thinking? If they are real, are they contemporaneous, and to what extent did one influence the other? As regards material products, differences between them are far easier to find than similarities, a point which is underlined by Donald Collier's paper at the same conference, in which he points out the almost complete lack of specific parallels between Peru and the neighbouring republic of Ecuador.

In both Peru and Mexico the Imperialistic or Militaristic stage was abruptly ended by the European invasion. It is interesting but unprofitable to speculate about what the next development would have been if this had not happened, but the study of what went before is of interest to everyone. The isolated position of Peru, and its development apparently undisturbed by outside influences, are in marked contrast with conditions in the Old World, where it would be hard to find a self-contained region which developed naturally without interference from its neighbours. If the natural sequence is from lowly agricultural communities, through small theocratic, peaceful states to great militaristic empires, then Peru provides an ideal example for study, as well as a warning. Such a

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general scheme of development is no new thing, as Kroeber points out in his summing up (5), but it has not been applied before to the New World.

Of the other papers read at the Conference, most are of interest mainly to Peruvian specialists. A summary of the Racial History of the Peruvian Area by Marshall T. Newman may be of rather melancholy interest to physical anthropologists, chiefly as showing the nakedness of the land in their particular field. An account by George Kubler, which he repeated in French in a masterly fashion at the 28th International Congress of Americanists in Paris, of an attempt to obtain an absolute dating for the Mochica culture, is of interest as a somewhat bizarre application of geochronology. The method is based on the depth at which objects were found buried in the guano, and depends on the assumption that it was deposited at a constant rate. This implies a constant bird population on the Guano Islands, and anyone who knows that region will readily believe that as many birds as there was room for were always accustomed to sit on the islands! Owing to the destruction of the guano, the results can never be checked, but the approximation obtained, the 7th to 11th centuries, agrees well enough with the guesses of most archaeologists.

⁵ Toynbee, A. J., *A Study of History*, 6 vols., 1933-9, London.

Customs and Traditions of the Isle of Portland Dorset

by ROBERT DOUCH

THE Isle of Portland reaches out into the waters of the English Channel. Almost a solid block of stone, it is the most southerly point on the Dorset coast (1). Its greatest length from north to south is four miles and its maximum width one and a half miles, while its entire circumference is less than nine miles. The north of the island is low-lying, but half a mile inland the ground rises steeply to a maximum height of 496 feet above sea-level at the Verne. From here it slopes away gradually to the southern tip or Bill, 20 feet above sea-level. There has never been a town of Portland and the chief centres of population were originally eight hamlets. Today, three of these, Castletown, Fortuneswell and Chesil have merged to form the main settlement in the north or Underhill, as the district is called. On Tophill three more of the hamlets, Reformatory, Easton and Wakeham have similarly run together. Weston and Southwell remain hamlets, while another settlement has grown up around the prison at 'The Grove'.

The 'Island' is, in fact, joined to the mainland by the Chesil Beach. But since this pebble bank extends westwards for ten miles before it meets the Dorset coast at Abbotsbury and could be used as a thoroughfare only with the greatest difficulty, the term 'island' is no real misnomer. Between Portland and the immediate mainland to the north runs the Fleet, a narrow arm of the sea, wide enough to make the approach across Smallmouth by ferry, before the building of the modern bridge, awkward and, at times, dangerous.

Until the middle of the 19th century the Portlanders seem to have had only limited contact with the mainland. Anyone not a native was a 'kimberlin' (2), and was treated with suspicion. Thus, it is not surprising that marriages were rarely contracted with outsiders. The solidarity of the island community is further emphasized by the fact that, as a rule, marriage did not take place until the woman was pregnant (3). If, after courtship, no conception followed, the couple separated, assuming that Providence looked unfavourably on the match. When pregnancy did occur, the two invariably became husband and wife (4).

¹ See fig. 1 Portland, co. Dorset, lat. 50° 33' N., long. 2° 26' W. The Ordnance Survey maps covering the Island are:—

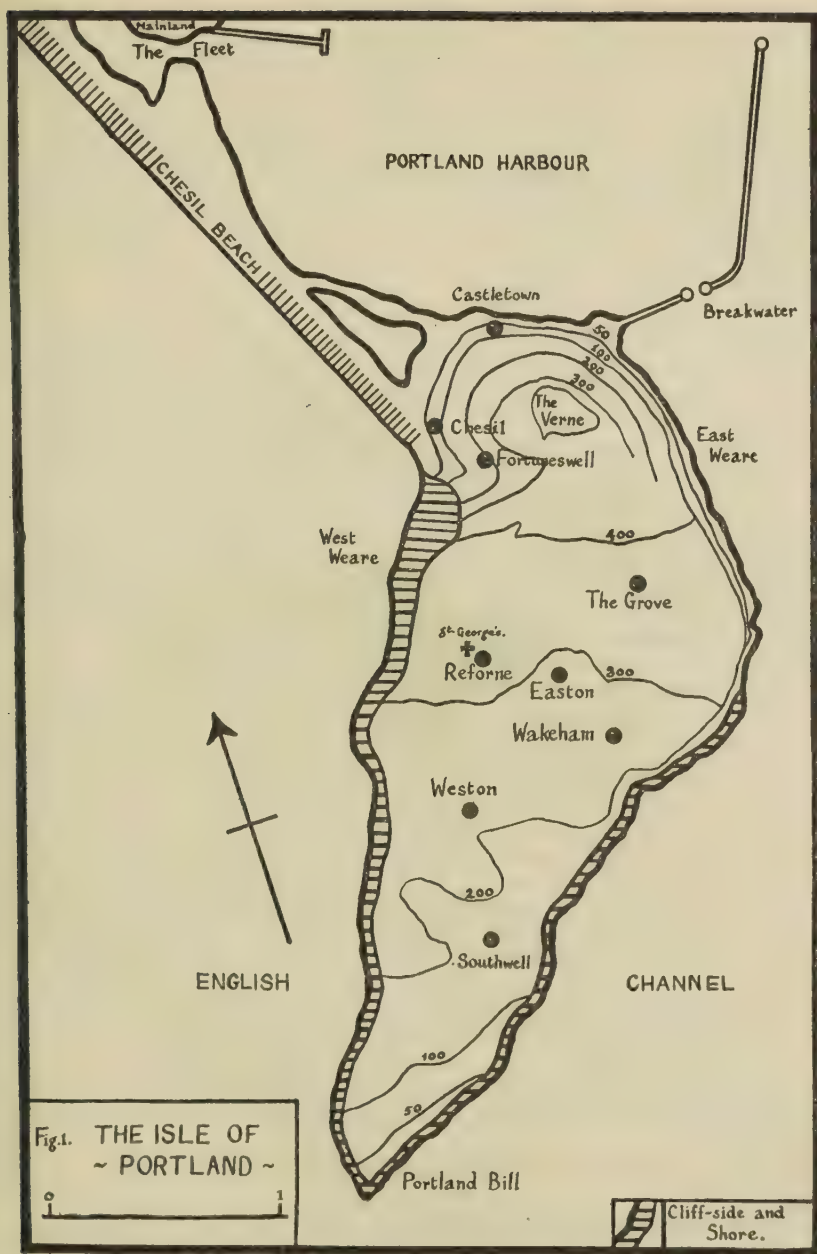
6-inch Sheets nos. 58 N.E., 58 S.E. and 60 N.E. (revised ed. of 1926 with additions of 1938).

25-inch Sheets, nos. 58—7, 11, 12, 15, 16 and 60—3, 7 (1929).

² Kimberlin or kimling, cf. *The English Dialect Dictionary*, ed. J. Wright, s.v. 'comeling'.

³ J. S. Udal, *Dorsetshire Folk-Lore* (Stephen Austin, Hertford, 1922), p. 199, suggests that this custom was not unknown in west Dorset and in Devonshire. It was perhaps connected with the old Danish marriage-contract known as 'handfesting': see J. Brand and H. Ellis, *Observations on the Popular Antiquities of Great Britain* (H. G. Bohn, London 1854), vol. II, p. 87. For a full description of the custom in Portland, see J. Hutchins, *The History and Antiquities of the County of Dorset* (J. B. Nichols and Sons, Westminster 1863, 3rd edition ed. W. Shipp and J. W. Hodson), vol. II, p. 809.

⁴ For the fate of some Westminster masons who broke this custom, see Hutchins, op. cit., pp. 820—1. They were stoned out of the island. Cf. p. 809: the birth of a deaf and dumb child to one of the unfortunate girls was held to signify God's wrath at the disregard of the custom.



Constant intermarriage is such a small community necessarily limited the number of patronymics in use on the island. In all classes of records the Pearces, Combens, Lanos, Attwoolls, Stones, Whites and Flews predominate. Accordingly, it has been the custom for nicknames to be used to distinguish one person from another. So customary was the use of these nicknames that they are sometimes found even in official documents (5), and a man unknown to his neighbour by his baptismal name, might be easily recognized if called by his nickname. Today, these soubriquets are fast dying out, but an authentic list of more than five hundred of them which were in use early in the 20th century has been published (6). The derivation of some is easily perceived—'Long Robert', 'Fat Chap' and 'Shoemaker Ned': but whence came 'Tea-pot Spout', 'Lord Zinc' and 'Sunday Morning'?

While the stone trade, especially during Wren's rebuilding of London, must have done something to break down this insularity, the Portlanders remained a race apart. Even in the 20th century there were still some who had never been 'to England'. The experiences of those who did go to the mainland probably modified the island customs little. However he behaved outside the island, when in Portland a man did as Portlanders did. As long as there was no considerable immigration, the force of local custom further emphasized and perpetuated the results of physical isolation.

The peculiar customs and traditions which survived were by no means confined to family names and to marriage. As Thomas Hardy noted in the preface to his novel, *The Well-Beloved*: 'The peninsula . . . has been for centuries immemorial the home of a curious and well-nigh distinct people, cherishing strange beliefs and singular customs'. Today, most of these have died out: those remaining are obsolescent. Thus it has seemed desirable to collect all relevant information, both from written evidence and from old Portlanders themselves, before all traces have disappeared (7).

In a district where farming and quarrying have been the basic industries, there are many survivals connected with agricultural organization and practice. The most important of these is the existence to the present day of Open Arable Fields. These and their significance in proving that in one part of England, at least, the dividing balk between strips was not 'the invention of prejudiced ignorance' have already been noted (8). Since I hope to deal in detail elsewhere with the agrarian history of Portland, it will be sufficient to notice but briefly some of the peculiarities of the agricultural organization of the island. Thus, until the passing of the Tithes' Commutation Act in 1836, tithes were paid in kind (9), and as late as 1843 half the arable land was left fallow annually (10). The islanders also had their own units of measurement. A 'yard' equalled a quarter of an acre, that is a plot one pole wide and forty poles long. This measure was not

⁵ See Dorset County Museum, Dorchester, Box File 'Dorset 5', for a transcription of names and nicknames as registered in the official Tithe Map and Commutation.

⁶ C. J. Samways, *Portlanders' Titles: a Souvenir* (Portland, 1937).

⁷ I would like to thank all those who helped me by supplying information, especially Mr John Pearce and Mr Richard Lano of Portland, Mr J. W. Warren of Weymouth and Lt.-Col. C.D. Drew of Dorchester.

⁸ See Charles D. Drew: Open Arable Fields at Portland and Elsewhere: *ANTIQUITY*, vol. XXII (1948), pp. 79-81.

⁹ R. Pearce, *Methodism in Portland* (Charles H. Kelly, London, 1898), p. 122.

¹⁰ W. Stevenson, *General View of the Agriculture of Dorset* (Board of Agriculture Report, 1812), p. 201, and R. Pearce op. cit., p. 125. But cf. Hutchins op. cit. II, p. 809. 'The arable land is mostly common field, which is divided into three parts or divisions, one of which is every year a fallow'.

unknown in other parts of England, but I have found no mention of an 'over-yard', equivalent to one and a half 'yards', outside Portland. The quarryman's foot contained twelve and a half inches, while the 'lug', used for measuring walls represented fifteen feet (11). These walls are another interesting survival. Built to enclose land, they sometimes replace the grass balks. But whereas the ownership of balks is laid down by custom and confirmed by law (12)—the southern lawnsbed or balk belonging to the northern lawn or strip, and the eastern lawnsbed to the western lawn, there is no custom defining the rights of ownership of walls. Each dispute over the latter has to be settled on its own merits.

The Court Leet, having survived the passing of Birkenhead's Law of Property Act (1925), still meets annually in Portland (13). The King is lord of the manor and his steward or Deputy calls the Court. Since there is no longer any communal cultivation in the island, the main business before the Court is not concerned with agriculture. Fines are still levied for encroachments on the waste and on the Commons, and when necessary, the Court negotiates for the sale of Common Lands (14), but practices connected with quarrying occupy most of the time.

Another important duty of the Court Leet is that of 'beating the bounds' of the manor. Surrounded as it is by water, Portland has the sea for its boundary except where it abuts on the neighbouring manor of Abbotsbury some four miles along the Chesil Beach. At this spot, a stone, 'Bound Stone', marks the division between the two. Thither, once every seven years, on Ascension Day, go the Court Leet officials to beat the bounds and, if necessary, to renew the stone. The Parish Accounts show that in the early 19th century cakes and beer were provided when the 'cullars' were carried to 'Bound Stone' (15) and ample refreshments are still a marked feature of the ceremony.

One of the most important manorial officials was the Reeve. In Portland the Reeve was appointed at the Michaelmas Court to serve for one year and was always that tenant paying the highest quit-rent who had not held office before (16). A woman could be Reeve. Mrs Rebecca Lowman, for example, held office in 1831, and three women, Margaret Heath, Margery Comben and Margery Spencer, were successively Reeves for the years 1845-7. But no one could be Reeve for a second term. Until 1883, the holder of the office was granted the use of a piece of land called the 'Reeve Plot' in return for his services (17), but after that date a money payment was substituted.

¹¹ Dorset County Museum, H. J. Moule's Notebooks, vol. VIII, pp. 173-4.

¹² See the judgment of Mr Justice Branson at the Dorchester Assizes, June 1935, in the case *Radford v. Mitchell and Peavitt*. The proceedings were fully reported in *The Dorset Daily Echo*, the decision being given in the issue dated 10 June 1935.

¹³ It previously met twice a year. For a full description of the Court, its officers and oaths, see: J. W. Warren, *The Island and Royal Manor of Portland* (Butler and Tanner Ltd., Frome and London, 1940), pp. 7-34.

¹⁴ In 1847 the tenants of the manor received £20,000 as compensation for the loss of their rights on the Great Common, when the Breakwater and Prison were built. Fines are also paid for the erection of telegraph poles on roadside waste.

¹⁵ See *The Parish Book of Portland, 1779-1820*, p. 89 et passim. Mr J. W. Warren kindly lent me this book.

¹⁶ See Public Record Office: Augmentation Office, Parliamentary Surveys E.317/12, which lists the tenants and their rents and states the customs of the manor in 1650, cf. P.R.O.: L.R. Misc. Books, no. 214, pp. 93-150 for a rent roll and less detailed statement of customs for 1608.

¹⁷ For its location and area see the Tithe Map and Apportionment Award, Portland, Dorset 10/173, parcel no. 306. These are held by the Tithe Redemption Commissioners, Finsbury Square, London.

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It was one of the chief duties of the Portland Reeve to act as collector of the rents. The quit-rent paid to the King for the whole of the island amounted to £14 14s 3d. Threepence an acre was paid for land, a farthing for the site of a cottage and a halfpenny for a cottage with a garden. The rents of all tenants were entered by the Reeve on a Reeve-Pole or Reeve-Staff (18). This Reeve-Staff was a pole of mahogany, deal, or pine, from one to one and a quarter inches square and varying in length from seven to twelve feet. On it were symbols representing the five tithings and the amount of rent to be paid by each tenant. A 'V' represented Chesil, a 'W' Easton, a hollow circle Southwell, a circle with a cross in it Wakeham, and a cross between parallel lines Weston (FIG. 2). Notches and scratches signified the rent owed. Thus a whole notch right across the face of the pole equalled a shilling and half a notch sixpence. Similarly a full scratch represented a penny, half a scratch a halfpenny and a quarter of a scratch a farthing. Dots separated one amount from the next (FIG. 3). In order to shorten the

V	CHESIL	○	SOUTHWELL
W	EASTON	⊗	WAKEHAM
⊠	OR	⌵	WESTON

Fig. 2.
Diagram of Tithing-Marks on Reeve-Staff.

pole, a new method of cutting was introduced as late as 1885. Under the new system, a diagonal notch, which had not been used before, equalled a pound. A straight notch still signified a shilling and half a notch sixpence, but now there were also three-quarter and quarter notches representing ninepence and threepence. Single pence and fractions of a penny were shown as before. The Reeve Staff had to be produced at Court Leet as evidence of collection and payment: it was then returned to the Reeve. Many old staffs are still in existence and are highly prized by the descendants of the original holders. There are four in the Portland Museum, and others in the County Museum at Dorchester (19).

Portland not only retains traces of the medieval system of cultivating the land: it was also unique in its methods of land-transference. A 19th century custumal states:

¹⁸ 'The tenants hold their lands . . . by custom of a Staff, and not by Copy, the rents being set on the said Staff, and every year the Reeve for the time being is to increase or decrease every Tenant's Rent, by making a new Staff . . .', P.R.O., E. 317/12.

¹⁹ For a full description see F. W. Galpin, *The Portland Reeve Staff. The Reliquary and Illustrated Archaeologist*, April, 1903, pp. 73-80 and 216.

CUSTOMS AND TRADITIONS OF THE ISLE OF PORTLAND

'We also present that we have three ways to dispose of our lands, namely in free church gift, Surrender at Court, and last will and testament' (20). The last method needs no explanation.

When land was conveyed by Surrender at Court the two parties attended the Court Leet, where the holder surrendered the property in question to the lord. Then, when the price had been fixed and a fine, called a 'Landchief' and equal to the quit-rent paid on the property, had been handed over, the buyer became the new tenant. Thus, for example, on 25 October 1731, 'To this Court came John Peters and surrendered into the hands of the Lord King . . . one close of pasture called Colecroft . . . with all its rights . . . for the use of Andrew Stone, his heirs and assigns forever. Whence there fell to the Lord King as a Landchief twopence farthing. And . . . the Lord, through his Seneschal, gave him seisin by the staff'. The rent to be paid was twopence farthing and the purchase price was £40 (21). If the heir of the seller wished to redeem land thus sold, he could do so by paying the purchase price to the buyer before two or more tenants of the manor within a year and a day of the original transaction (22). Surrender at Court was still used as a means of conveyancing in the 19th century. In

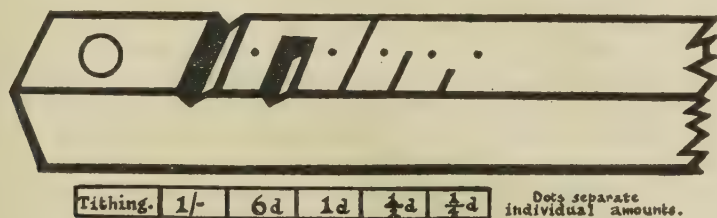


Fig.3.—Diagram of Reeve Staff.

1821, when the lands of Mr John Gilbert were sold, it was declared 'That the purchase deeds of the several lots shall be prepared . . . at the expense of the respective purchasers, unless they shall choose to take the same by Surrender in Court, at their own expense (23)'.

In the Middle Ages the Parish Church was often used for business transactions (24), and the third way of transferring land in Portland was a survival of this practice. Transference of property by Church Gift continued until 1925 (25). By this method

²⁰ See *The Historic Court of 1846*, printed in Warren, op. cit., p. 19. The two 17th century surveys also describe the custom.

²¹ This example, a translation, is taken at random from the Court Rolls which are almost entirely composed of these Surrenders. The entries are in Latin until 25 October 1732, and thereafter in English. The rolls cover the years 1670-1773: P.R.O., L.R. 3/17/1-4.

²² P.R.O., E. 317/12.

²³ This sale was one of the largest the island has known. Mr J. W. Warren kindly lent me his copy of the original Bill of Sale.

²⁴ See S. O. Addy, *Church and Manor: a study in English economic history* (Geo. Allen and Co. London, 1913). There is a small room on the south side of the parish church of Breamore, co. Hants, with an arch bearing an inscription in Anglo-Saxon: 'Here the agreement is made'.

²⁵ The last entry in the Register of Church Gifts in the Parish Chest of St. John's, Portland, is dated 30 July 1925. Unfortunately the record does not begin until 1890.

the parties concerned met in the parish church and with at least two tenants of the manor as witnesses transferred the property from one to the other. Originally the conveyance was purely verbal. The extent of the land was declared, the parties joined hands, the priest laid his hand on theirs and the transaction was complete (26). In the 17th century (27) a father who wished to leave land to his daughters stood in the church porch after evensong and in the presence of the congregation named his daughters in turn, specifying the exact boundaries of the lands which each was to inherit. After this the congregation would rise up and bless the daughters by name. Although the term 'Church Gift' is not mentioned, it seems clear that this is the custom described. By this time the presence of the priest was no longer necessary. At a later date the agreement was often committed to paper, but it was not until 1845 that the law demanded that a definite deed be made. After 1845 it was usually the practice for the Church Gift to be drawn up, sometimes by the schoolmaster, taken to the Church, and signed in the presence of two witnesses (28). The form of the document varied little: 'Memorandum That on the . . . day of . . . in the year . . . I . . . of Portland, in the county of Dorset, came into the parish church dedicated to St. George, and did then and there, according to an ancient custom of the island, time out of mind, in the presence of the undersigned witnesses freely give unto . . . And this I acknowledge to be my free Church Gift. In witness whereof I have set my hand and seal this said day and year above written' (29). A register, in which these transactions were to be entered, was kept in the church, but it was not always used since many people wished to keep their business private. Old Portlanders still strongly resent the abolition of this cheap method of land transference by the Law of Property Act of 1925.

In conjunction with these customs of land transference, it is interesting to note that long before the passing of the Married Women's Property Acts the wife of a Portlander could inherit property in her own right and bequeath it by will independently of her husband (30). Moreover, by marrying a Portland girl who was a tenant of the manor, a man became a freeman of the island with the right to work in the quarries.

A peculiar method of distributing property as well as of conveyancing existed in Portland. It has been suggested that the tenure of gavelkind obtained in the island (31). This suggestion seems to be incorrect. The custom of gavelkind strictly refers to tenure only, that of holding land in return for the payment of food rents. Certain customs often accompanied this tenure. Thus land descended upon intestacy to all sons equally, a widower was entitled to half his wife's land so long as he remained unmarried, a widow received half of her husband's land as a dower, a felon's heirs could inherit his father's lands, an infant could alienate his land when he reached the age of fifteen, and the land was devisable (32).

²⁶ For some account of this custom, 'marrying the land' as it was called, see *Somerset and Dorset Notes and Queries*, no. 4, p. 62.

²⁷ See British Museum, Stowe MSS. 597, fol. 42b et seq.

²⁸ See *Notes and Queries*, 9th series, vol. VIII, p. 81.

²⁹ See e.g. Dorset County Museum, Church Gifts, dated 1770, 1842 and 1861: nos. 11,061, 11,060, and 11,058.

³⁰ See Hutchins, *op. cit.*, vol. II, p. 809.

³¹ See e.g. Hutchins, *op. cit.*, vol. II, p. 811, quoting J. Smeaton, *History of the Eddystone Lighthouse*. (2nd edn., London, 1793).

³² T. Robinson: *The Common Law of Kent or the Customs of Gavelkind* (3rd edn., London 1822), Part II.

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In Portland partible inheritance was the custom in cases of intestacy. But the division of land amongst all sons equally when a person died without leaving a will, was only one of the incidents of gavelkind. Thus the custom of partible inheritance did not mean that the custom of gavelkind prevailed in that district. Furthermore, a widow in Portland received only a third of her husband's land, whereas in Kent she would have received a half. At the most we can loosely say, in the words of the 17th century survey, that in Portland land was 'descendible in gavelkind'. When a man or woman died intestate, the sons shared the land equally: daughters could not share in the inheritance unless there were no sons (33). Whether gavelkind was ever the custom in Portland as it was in Kent, I have found no evidence to decide. It is perhaps significant for the medieval organization of the manor that the Domesday Book entry for Portland lists five slaves, only one villein and ninety bordars.

Although land which was partible might be split up amongst its owners, it seems that the economic advantages of maintaining the property undivided were early realized (34). In Portland, actual division would have led to chaos. Individual strips were already small and further sub-division would have resulted in the creation of minute parcels of land useless as economic units. Property was therefore held in undivided shares. In 1821, John Gilbert offered for sale amongst other properties, 'five undivided ninths of a piece of arable land in the South Field by estimation 1 rood 13 perches (35)', and an entry in the Church Gift Register refers to the transfer of 'one seventh of a fifth part of a land in Yeolands' (36). Although partible inheritance was ended by the Law of Property Act (1925), many Portlanders still own undivided shares. These divisions do not affect cultivation, but they are of real importance. If land is sold all owners have to be traced. Before undivided land can be quarried the consent of every owner has to be obtained, though if land once opened is left for a while, only the permission of the largest part-owner is required to recommence working (37). Furthermore, if stone is quarried from an undivided strip, duty has to be paid to the several owners. Similarly the road dues (38) have to be shared.

Quarrying, like agriculture, furnishes many examples of curious customs and survivals. This industry is now of national importance, and its history and organization, which demand special attention, can only be briefly noticed here (39). While Portland quarries were worked for local purposes and for limited export in the Middle Ages, no large scale quarrying took place until the 17th century. Difficulties of digging the stone, which often lay thirty feet below the surface, poor transport facilities and the lack of any real demand prevented their development (40). Building in London, under the direction of Inigo Jones and more especially of Sir Christopher Wren, laid the real foundations of the industry.

³³ P.R.O., E. 317/12.

³⁴ See J. E. A. Jolliffe, *Pre-Feudal England: the Jutes* (Univ. Press, Oxford 1933), p. 20.

³⁵ Bill of Sale, see n. 24.

³⁶ Register of Church Gifts, no. 121.

³⁷ Ex. inf. Mr J. Pearce.

³⁸ See below.

³⁹ See *Victoria County History of Dorset* (Constable, London, 1908), vol. II, pp. 338-43. I hope to deal in detail with this subject later.

⁴⁰ See D. Knoop and G. P. Jones, The English Medieval Quarry, *Economic History Review*, vol. IX (1938-9), pp. 22-4.

Most of the early quarries were on Common or on Crown Land, and only tenants of the manor could open and work them. In 1664, the islanders petitioned the king for a duty of twelvecence per ton to be levied on all stone exported from the island (41). This duty which was already imposed by ancient custom (42) was to be shared equally between the king and his tenants. Charles II was more generous than the islanders had asked. In return for the damage done to the Commons by quarrying and for the loyalty which Portland had shown during the Civil War, Charles, in 1665, granted ninepence of the duty on every ton of stone from the Commons to the islanders, and kept only threepence for himself (43). For every ton of stone raised in the demesne lands 'time out of mind' threepence was paid to the inhabitants until Queen Victoria increased the amount to sixpence. If the demesne lands were leased, the farmer received sixpence of the Crown's share. Stone dug from the Commons by the tenants for their use within the manor or from the demesne by the lord for his own use paid no duty. Similarly tenants could raise what stone they wished in their own lands free of charge (44).

Besides this duty of a shilling a ton, there were other dues to be paid. On the east and west side of the island are the 'Weirs' (45). Below the cliff top and stretching down towards the sea, the weirs were formerly the dumping ground for the rubble which has to be cleared before the marketable Portland stone is exposed. Rubble from quarries on the Commons could be deposited free of charge, but stone merchants had to pay sixpence on every ton of wrought stone for rubble which was dumped from private grounds (44). The East and West Weirs were divided into smaller weirs: thus, for example, in the Parish Book, Green, Knight's, Church Point, Hind's and Gregory's Weirs are noted. These weirs were often rented for their profits, and like land, were frequently held in undivided shares. Thus in 1911 fourteen people had an interest in Gregory's Weir: the Bath and Portland Stone Firms Ltd., holding $\frac{7}{12}$ and $\frac{1}{200}$ were the largest shareholders and Mr W. Comben (Prospect Place) with $\frac{1}{1200}$ was the smallest (46). In 1937 the last payments for depositing rubble were made. Quarrying activities have moved inland and the overburden is no longer sent to the weirs. When quarrying near the cliff-edge is resumed, rubble dues will undoubtedly be paid again.

Rubble dues are temporarily obsolete, but twopence per ton still has to be paid to the landholders over whose property stone passes on its way from the quarries to the road. Early in January each year, the quarry owners furnish the assessor with details of the amount of stone dug in their quarries during the past twelve months (47). With the help of a map, the latter decides who are entitled to receive these road-dues, and on the 30 January the money is shared out. In 1930 the record amount of £617 16s 1d was

⁴¹ *State Papers Domestic*, 1663-4, p. 618.

⁴² See Answer to Article of Enquiry no. 10, 1 June 1678: L.R. 3/17/1.

⁴³ *S.P.D.*, 1665-6, p. 41. This royal grant has to be renewed at the accession of each king. The extra threepence on every ton granted in 1665 is paid into 'His Majesty's Grant Fund'. This money, vested in trustees, is used to help finance schemes for the general welfare of the island. Two poor houses and St. George's Church received grants from this fund.

⁴⁴ See *The Historic Court of 1846*, printed in Warren, op. cit., p. 20.

⁴⁵ Weir, wear, weare, ware: Hutchins, op. cit., p. 822, note a, suggests that the word is of Celtic derivation and means 'rough, wild ground'.

⁴⁶ The late Mr J. Waight of Portland kindly supplied these figures. I have not been able to discover how such minute sub-divisions came into being.

⁴⁷ It is not known when this custom originated. Mr J. Pearce has for many years now allocated the road dues and holds the records of the payments made since the middle of the 19th century.

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divided in this way : in 1947, as the industry began to recover from the effects of the war, the sum to be allocated was £200 2s 2d. This division is no simple task. There is only one twopence for each ton : it is twopence to be shared amongst all the landowners, not twopence for each of them. Moreover the number of strips over which the stone has passed may be considerable, and there is always the added complication of undivided shares.

There is another important island custom concerning roads or 'ways', as they are called. If a public road is destroyed by quarrying operations, it is the custom for the proprietor of the adjoining property to allow a way through his land. In return for this concession he receives the tonnage dues for all stone raised under the old road (48).

The introduction of modern machinery has done much to alter methods of work in the quarries. But until the middle of the 19th century they had probably changed little since the days of Wren (49). Manual labour still did most of the work, horses were still the main form of transport (50), and export was still mostly by sea. The quarryman still received only a small weekly allowance and a full scale settlement only took place quarterly (51). But the mechanisation of the last hundred years has wrought great changes : although old tools, such as the 'kevil', half hammer and half-pick-axe, are still used, the mechanical excavator, the electric drill and crane, the lorry and the railway have revolutionized the old organization.

Any social customs which characterized quarry life have likewise disappeared. Of them, no record is left. There is one exception : we have full details of the celebrations which attended a quarryman's marriage (52).

Before the changes of the late 19th century, quarrymen usually worked in gangs of four men and a boy in each quarry. On the morning of his wedding, the bridegroom would send a cake, a bottle of whisky, a bottle of port-wine, a bottle of peppermint and about a gallon of ale to his workmates. When this arrived in the quarry, a flag was hoisted on the mast of the crane as a sign that all was well and inviting those working nearby to join the celebrations. Festivities continued all day, a favourite pastime being 'Rattlehat'. In this game, five pennies were placed in a hat, and the gang which threw most 'heads' was the winner. Fresh supplies of liquor were regularly obtained. Before the bridegroom started work again he visited his quarry and 'jumped'. This ceremony was usually conducted by the oldest man amongst those celebrating the marriage. He appointed two tellers, whose duty it was to take the name of anyone who failed to observe the rites. Then, all hats having been removed, preparations were made for the 'jump'. The man last married held one end of the 'jumper' (an iron rod used in the quarry for

⁴⁸ *The Historic Court of 1846* : see J. W. Warren, op. cit., p. 26.

⁴⁹ See R. Damon, *The Geology of Weymouth, Portland and the Coast of Dorset* (New and enlarged edition. Weymouth and London, 1884), p. 94.

⁵⁰ An important exception to this was The Merchants' Railway. Opened in 1826, it was the first railway in Dorset. It was used to convey stone from Top-hill to Castletown Quay. It worked, with drums and cables, by gravitation, the loaded trucks going down causing empty ones to ascend on another set of rails.

⁵¹ Dorset County Museum, H. J. Moule's Notebook, vol. ix, pp. 146-9, shows that the quarterly settlement was still the custom in 1896. In the 18th century it was a half-yearly settlement : see E. L. Blanchard, *The Watering Places of England : Weymouth* (W. J. Adams, London, 1850), pp. 46-7.

⁵² I am indebted to Mr B. R. C. Smith of Southwell, Portland, who kindly gave me a description of the ceremony as he witnessed it when a boy.

drilling holes) and the man who was supposed to be married next held the other end. All the married men lined up behind the former and all the single men behind the latter. Fines were imposed by the tellers for being out of place, for wearing a hat and for talking. Then the M.C. recited the service :

‘ Young men and bachelors, I bid you all adieu,
Old men and married men, I’m coming on to you ’.

Over the rod went the victim, helped by two of his workmates with sticks or shovels. Then he jumped back again to the words,

‘ Old men and married men, I bid you all adieu,
Young men and bachelors, I’m coming back to you ’.

Finally he jumped for a third time, as the first verse was recited again. Thus the ceremony ended and the newly-wed joined the ranks of the married men. Fines were counted and the celebrations continued.

Besides inheriting strange customs in the organization of their everyday affairs, old Portlanders clung firmly to numerous traditions, beliefs and superstitions. Tradition said that the island had been peopled by a three-fold settlement: ‘ first came the Combens : then from Ireland, by way of Cornwall, the Pearces, and thirdly the Whites who came from the sea, Dover way (53) ’. According to old Portlanders there had never been any peasants on the island (54), and at the other end of the social scale no one could be a gentleman if he or his ancestors to the third generation had been traders or had owned less than five hundred acres of land (55). It was also believed that ‘ Bow and Arrow ’ or ‘ Rufus ’ Castle, now situated on the very edge of the cliff on the south-east of the island, was once in the middle of the isle, and that there was formerly a butcher’s shop on the now dangerous stretch of water off the coast, the Shambles.

A firm belief in the supernatural accompanied the acceptance of these traditions : animals and men were endowed with more than earthly power. Today many remember men and women who could bewitch their fellows. There was, for example, ‘ Wold Maremaid ’, the keeper of ‘ The Mermaid Inn ’, whose very presence foretold impending disaster (56). Similarly women were not welcome when fishing was in progress since their presence was held to have a bad influence on the catch. The sight of a goat or a linnet was an evil omen, and it is still bad luck to call a rabbit by its proper name—‘ those furry things ’, ‘ Wilfreds ’, ‘ bunnies ’, or ‘ those long-eared things ’, but never ‘ rabbits ’. Even a tree possessed supernatural power. On Midsummer Eve 1823, a mother brought her young child from Weymouth with the intention of passing it through a cleft ash tree in the grounds of Pennsylvania Castle at sunrise to cure it of hernia (57).

Traditional ceremonies, most of which had died out by the middle of the 19th century, existed side by side with tradition and superstition. A Portland wedding was a colourful affair. After the ceremony the bridal couple walked round the island followed by their guests in pairs. Calls were made at friends’ houses and festivities were kept

⁵³ C. King Warry, *Old Portland Traditions* (Warren & Co., Portland c. 1914), p. 29. Documentary evidence suggests that this tradition is incorrect : Pearce is the most ancient island name which survives today.

⁵⁴ Mrs King Warry, *The Status of Peasantry in Portland : Proceedings of the Dorset Natural History and Antiquarian Field Club*, vol. xxx, 1908-9 (Dorchester 1909), p. 73.

⁵⁵ C. King Warry, *op. cit.*, p. 52.

⁵⁶ W. Sherren, *The Wessex of Romance* (Chapman and Hall, 1902), p. 90.

⁵⁷ Mrs King Warry, *The High Place* (typescript copy in Dorset County Museum Library), pp. 18-19.

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up for two or three days. One bride was followed by a hundred and ten guests, besides numerous spectators on her wedding day (58). St. George's Church had no peal of bells, so anvils, pistols and guns provided the musical accompaniment for the procession. An island funeral likewise witnessed a procession of couples. On this solemn occasion those attending were dressed in sad garments, and if the dead person were a child, the bearers would be young girls clad in white (59). A Portland funeral in the middle of the last century seems also to have been unique in that a funeral sermon was preached (60).

Besides these occasional ceremonies, there were also those which took place at fixed festivals in the year. Until late in the 19th century, the Wednesday of the seventh week after Christmas was 'Binding Day' (61). On that day, men and women took anything which they could from their neighbours, without intervention by the law, the trophies being redeemable on the payment of a small sum. May Day saw the cows turned into the Commons, processions in the streets with the girls dressed in white, dancing and general jollification. There was also the old custom of keeping the household fires alight, from November to May, and then forbidding them to be lit again (except for essential cooking purposes), from May to November (62). In the 18th century at Hocktide two Underhill women held a rope stretched across the road near Verne Yeats and every man who wished to ascend to Top-hill had to forfeit a kiss before he could pass (63). And on 5 November Portland Fair was held, but no records describing it seem to have survived.

Few of these traditions and customs exist today. Their survival depended on the isolation of Portland and on the absence of any large influx of population. As Hutchin's story of the Westminster masons shows, the force of local custom was strong enough to permit the absorption of a small number of immigrants or to enforce their expulsion if they broke the customs of the island. But as soon as the island came into close touch with the outside world it was inevitable that these customs would soon disappear. The middle of the 19th century marked the turning point. The building of the prison and the Breakwater and the construction of military fortifications on the Verne led to a great influx of population. Whereas the population in 1801 was under 2,000, it had arisen above 15,000 by 1901 (64). At the same time improved communications with the mainland (65), and better transport facilities generally meant that more people went to and from the island. Portland's isolation was at an end, and the old customs were doomed.

In 1949 the Court Leet, the tonnage and road dues on stone and the skeleton of the Open Fields still remain. The bounds are beaten, a few nicknames are heard, and a symbolic Reeve-Pole is still cut. Some of the old Portlanders believe in the

⁵⁸ Fido Lunettes, *An Historical and Descriptive Account of the Peninsula of Portland* (S. McDowell, London, 1825).

⁵⁹ Mrs King Warry in T. Perkins and H. Pentin, *Memorials of Old Dorset* (Bemrose, London, 1907), p. 186.

⁶⁰ See J. Brand and H. Ellis, *op. cit.*, vol. II, p. 279.

⁶¹ See A. M. Wallis, *The Portland Stone Quarries: Proc. Dorset Nat. Hist. and Antiq. Field Club.*, vol. XII, 1890-1 (Dorchester 1891), p. 194. The origin and significance of this custom, like those of many which follow, are obscure.

⁶² An old man who died c. 1830 was the last to adhere rigidly to this custom: see Mrs King Warry in Perkins and Pentin, *op. cit.*, p. 186.

⁶³ Mrs King Warry: *The High Place*, p. 44.

⁶⁴ Census figures: 1801, 1,619. 1901, 15,199.

⁶⁵ In 1839 a bridge replaced the old ferry-boat on ropes.

supernatural and rabbits are still evil omens. But on the whole the old order has changed, as life in Portland has joined the main stream of development.

Isolated as Portland has been, the history of this particular island throws some light on wider historical issues. The existence of the grass balk between strips in the Open Fields is established beyond doubt for this English manor, and perhaps for all manors on the light, dry soils of the limestone and chalk country. The transformation of the balk into a lynchet where the soil is turned down-hill is also illustrated, and the theory that some lynchets, at least, are intimately connected with Open Field agriculture is thus proved (66). The Court Leet exemplifies in detail the working of this formerly widespread organ of local government. The Reeve-Staff suggests a primitive method of reckoning to help those who could not read or write and is reminiscent of the Exchequer tallies. Whether the custom of partible inheritance is in any way connected with a possible Jutish settlement west of the Hampshire Avon, I have no evidence to decide (67).

But it is the difference between Portland and the mainland which gives the study of the Island's customs and traditions its real interest and value: they represent the way of life of a unique community settlement. Here, on an island, separated by only a narrow stretch of water from the rest of Dorset, a distinct people with a distinct set of customs survived within living memory. In the words of Peter Mundy who visited Portland in 1635, 'A strange alteration between this and the Maine, the distance being soe small' (68).

⁶⁶ See Drew op. cit., pp. 80-1.

⁶⁷ See Drew op. cit., p. 80, n. 7. The entire question of the origins of the peculiar Portland customs I am unqualified to discuss. The hamlet settlements and the custom of partible inheritance perhaps suggest Celtic influence.

⁶⁸ N. M. Richardson, *The Travels of Peter Mundy in Dorset, 1635*, *Proc. the Dorset Nat. Hist. and Antiq. Field Club*, vol. XLII, 1920-1 (Dorchester 1922), pp. 48-9.

Canterbury Excavations, 1944-8

by SHEPPARD FRERE

ON 1 June 1942, occurred the German air-raid which destroyed about one-fifth of the old part of the City of Canterbury. It was soon realized that a unique opportunity existed of discovering something of the archaeological past of the city before rebuilding again concealed the wide areas now exposed.

It was not only an opportunity but a duty ; for modern rebuilding, with its deep foundations, would be bound to destroy a very large part of whatever evidence had survived until the present. The Canterbury Excavations Committee was accordingly formed by the initiative of the local Archaeological Society, and excavations under a supervisor lent by the Ministry of Works began in August 1944, and have continued three times a year up to the present.

Canterbury is one of the few Romano-British towns where there are good grounds for supposing a continuity of occupation through the Dark Ages (1). Thus though there was little to hint at any pre-Roman occupation, from the Roman, Saxon, and Medieval periods much was to be expected. Yet such a prediction has not turned out wholly true. On the one hand evidence for a pre-Roman Belgic settlement is accumulating ; but little of Saxon date has been found, and the circumstances of the excavations have limited the extent of the medieval discoveries.

The town could not be investigated purely as an archaeological problem, digging on sites which seemed promising and following out the plan as on an open site like Silchester or Verulam. There was a double call. Rebuilding was ever imminent, and the first sites to be rebuilt were likely to be along the main street-frontages. Attention was necessarily devoted first to them.

The work of Mr W. Urry on the Cathedral Rentals has shown that much of the existing town plan, and many even of the existing property boundaries along the principal streets, go back at least as far as the 12th century. Almost all the main streets, Burgate Street, S. George's Street, S. Margaret's Street, and Watling Street, have cellars along their entire frontages. And so these cellars, mainly excavated in the 16th-18th centuries, have removed the medieval layers inside the houses, and often even the medieval walls themselves have been replaced. In the event, therefore, the medieval remains have been mainly confined to rubbish pits of various purposes, the contents of which are limited to broken pottery and bones. Few traces of medieval building have been found.

It is consequently with remains of the Roman period that the excavations have principally been concerned ; and this is as it should be, seeing that the Roman period constitutes one-fifth of the history of the city, and about it least is known. The medieval history of Canterbury is well documented, and it may be that the most valuable result of the Excavations as regards medieval studies will be in the production of a dated series of pottery types.

Medieval pottery is very regional in its designs, as is now well-known ; and it will be useful to have a large Kentish series. But even here there are difficulties, for it is but rarely that the pits produce any object of external dating value, and the dating of coarse pottery is still too largely subjective.

¹ See Collingwood and Myres, *Roman Britain and the English Settlements*, 428 and *passim*.

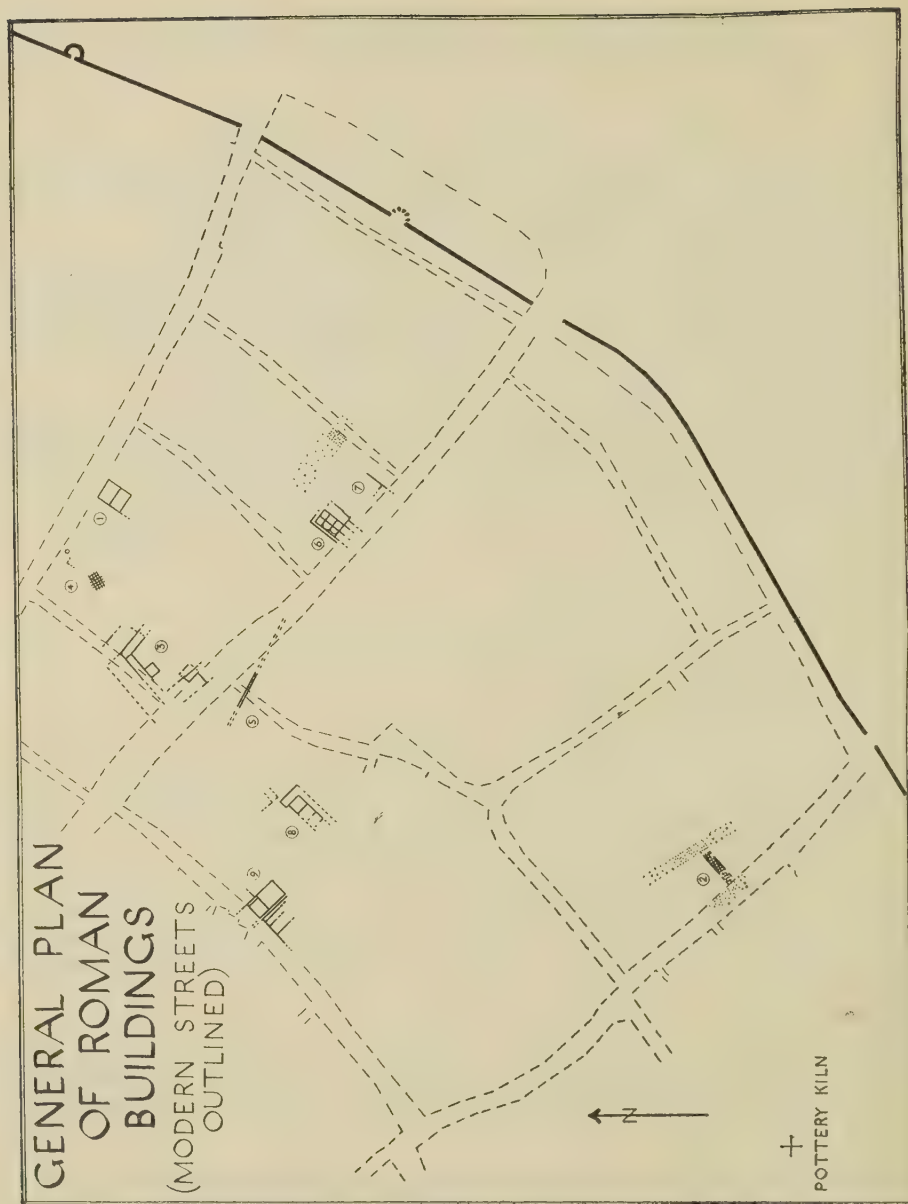


FIG. 1. ROMAN BUILDINGS IN CANTERBURY

That Canterbury was the Roman city of Durovernum Cantiacorum has long been established; but having been in continuous occupation ever since, little was known either of its history or town plan: that little has recently been summarized (2). Our principal source of information was the account published by James Pillbrow in *Archaeologia* XLIII of his observations when trenching the streets in 1867 for main drainage. He was in advance of his age, by British standards, to have noted anything at all; but it cannot be said that his article leaves us very much wiser, partly owing to the small scale of his plan, and partly owing to his failure to notice the Roman streets as distinct from the house-walls. But at least we can be certain of two things; first that some of the chief buildings lay under the High Street near the County Hotel west of the area now laid bare, and secondly that wherever the Roman streets were, they do not underlie their modern successors.

It must be confessed that the Roman street-plan still eludes us. This is due partly to the fact mentioned earlier that we have been digging systematically in cellars, not laying down long trenches as over an open site, and partly to the prevalence of medieval rubbish pits in the cellars. The problem of soil-disposal in a small cellar is always acute, and a four-foot trench might easily cross the line of a Roman street, yet be wholly occupied by a series of deep intersecting pits of later date. Only when the time comes to trench across the wide spaces behind the cellars may we hope to learn something coherent of the streets.

And here another factor, the great depth of deposits, must be considered. The height of 'natural'—that is undisturbed—soil about sea-level varies considerably from site to site. In Burgate Street the cellars are about 7 feet below street level, and natural soil a further 8 feet lower, at 30 feet O.D. At the north end of the Butchery Lane cellar it was even lower, at 24 feet O.D. But it rises towards the South, so that in parts of S. George's Street it is not more than 2 feet below cellar floors (35 feet O.D.), unless a pit is encountered.

Hitherto the work has almost all been done by volunteer helpers, schoolboys and schoolgirls, university students, and others on holiday. Paid help has rarely extended beyond breaking some of the concrete cellar floors. The reason has been that large-scale rebuilding has been continually put off, and it was felt that as long as we could carry on in this way, other cities—Dover, London, Southampton, Exeter—with more urgent claims should have the field clear for appeals.

But this scheme of things has prevented much digging from the surface, except when a Roman building was known to exist and a surface-trench was necessary to complete the plan. Few volunteers, and those only the most muscular, can be expected to dig regularly at ten feet down. It is evident that money on some scale will have to be expended—and first collected—if we are to cut hundred-yard trenches from street to street.

Some help can be got by the mechanical excavation of the upper levels once these are known to be of little value; but there is always the danger of thus destroying important Saxon or medieval remains unless soundings are taken first. A start, however, was made in 1948 in the yard of the Rose Hotel, west of Rose Lane, where an earlier trench had uncovered part of a 2nd century house covered by six feet of 18th century earth; here a wide area was lowered five feet by a grab, and parts of two other buildings discovered; though once again the problem of soil disposal became acute.

The time has now come, however, for work on a larger scale. The rebuilding of big areas of derelict ground is in sight, and the Excavation Committee has issued an appeal for £5,000 in order that paid labour and proper equipment may be employed.

² See *Roman Canterbury No. 1, The City of Durovernum*, by Sheppard Frere, published for the Excavation Committee by the Medici Society.

ANTIQUITY

THE BUILDINGS

The town, about 130 acres in extent, seems to have been of the garden-city type, resembling Silchester with its scattered buildings more than the crowded plan of Caerwent. At the time of the Conquest, Belgic occupation seems to have been concentrated on the crest of a slight rise of the underlying loam between S. George's Street and Watling Street (as far as is known at present).

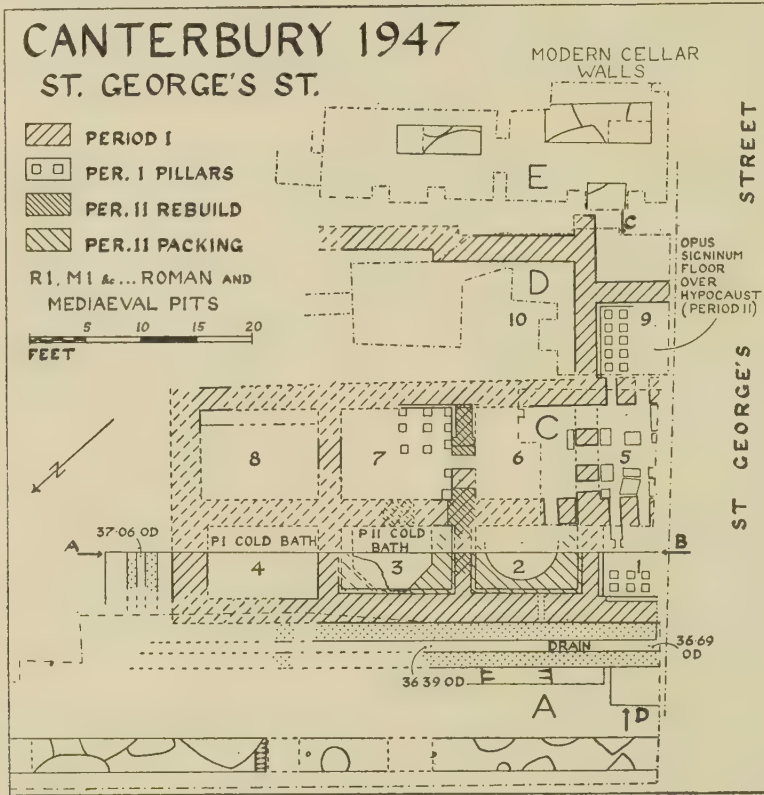


FIG. 2. PLAN OF BATH HOUSE IN ST. GEORGE'S STREET, CANTERBURY
(Block kindly lent by the Society for the Promotion of Roman Studies: see *J.R.S.* xxxviii, 99)

The earliest Roman phase in Claudian times, and subsequently under the Flavian dynasty, was embodied in timber buildings, of which sleeper beams and post-holes have here and there been found. The depth at which these have appeared has prevented wide excavation, and we know these periods in vertical section rather than horizontal plan.

Masonry replaced timber about the turn of the 1st century, and the ordinary houses were built in flint and mortar with corners turned in tile; but Kentish rag was used both now and later in more substantial structures.

No building has been recovered in its entirety, for they all either run out under a modern street or else disappear beneath a neighbouring undamaged property. It is interesting to see their relationship with more recent buildings. In two cases, the

CANTERBURY EXCAVATIONS, 1944-8

Butchery Lane house (FIG. 1, 3), and the house beneath the Rose Hotel Yard (FIG. 1, 8), they make no contact with their successors. In the Rose Yard the building had been well robbed in the late 4th century, and in Butchery Lane there was evidence to show that one Roman wall had been dug out in the later middle ages, while the rest had been cut down when the cellars were dug. But in three other cases things were different. In Burgate Street (FIG. 1, 1) one wall was still standing to within two feet of the surface, and had been in use throughout medieval and recent times though buried and encased; while the S. George's Street Bath-house (FIG. 1, 6) and the S. Margaret's Street Bath-house (FIG. 1, 9) both showed that the cellar-diggers of the 17th or 18th century, if they found a convenient Roman wall would build upon it, even if it meant taking a narrower line for their basement. This is well seen in the former site (FIGS. 2 and 3). The excavators of cellar A

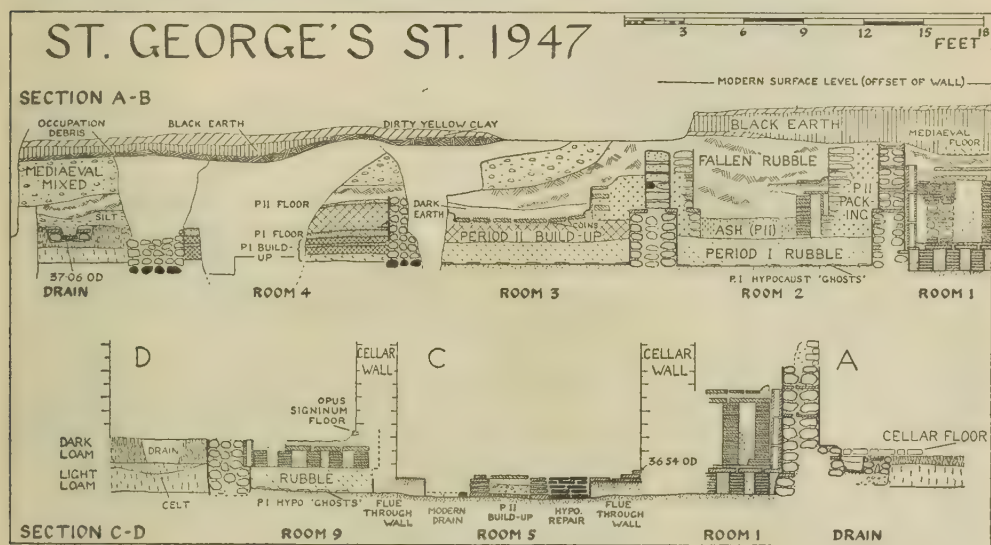


FIG. 3. SECTIONS OF ST. GEORGE'S STREET BATH HOUSE, CANTERBURY
(See FIG. 2)

struck the west wall of the Baths, still standing four feet high, and used its external face as the wall of their cellar, though this was four feet short of the property boundary. The men who dug cellar C cut through the second-period Roman hypocaust, the bricks of which they re-used in their cellar wall, still to be seen; and they used as their floor the tiled basis of this hypocaust, covering the blocked remains of its predecessor.

In cellar D, on the other hand, they did not sink so deep, and encountering a Roman *opus signinum* floor they re-used it. Unfortunately this floor rested on a hypocaust. Continual shovelling of coal wore down the concrete, and here and there holes appeared, leading to our discovery of unburnt coal in the Roman hypocaust!

The east wall of the Building crossed the neck of the cellar, whose wall then followed its line. Here, however, the excavators did not build on the top of the Roman wall, but masked it with their brick wall (FIG. 4). The reason perhaps was that it was not standing so high above their floor level as the west wall had stood in cellar A.

In the rear part of the site, north of cellar C, the building is still standing massively

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to within three feet of the surface except where destroyed by medieval pits. Indeed along section A-B the same is true, and the tiled floor of room 1 was only four feet down.

This leads us to wonder what was the appearance of Durovernum in Saxon times? One of the puzzling things about our excavations has been the curious absence of Saxon and Jutish objects. In many of the sections dug from the surface there is an abrupt transition from late Roman to 12th and 13th century deposits, with no trace of any intervening time. Until January 1948 our excavations had produced but one Saxon

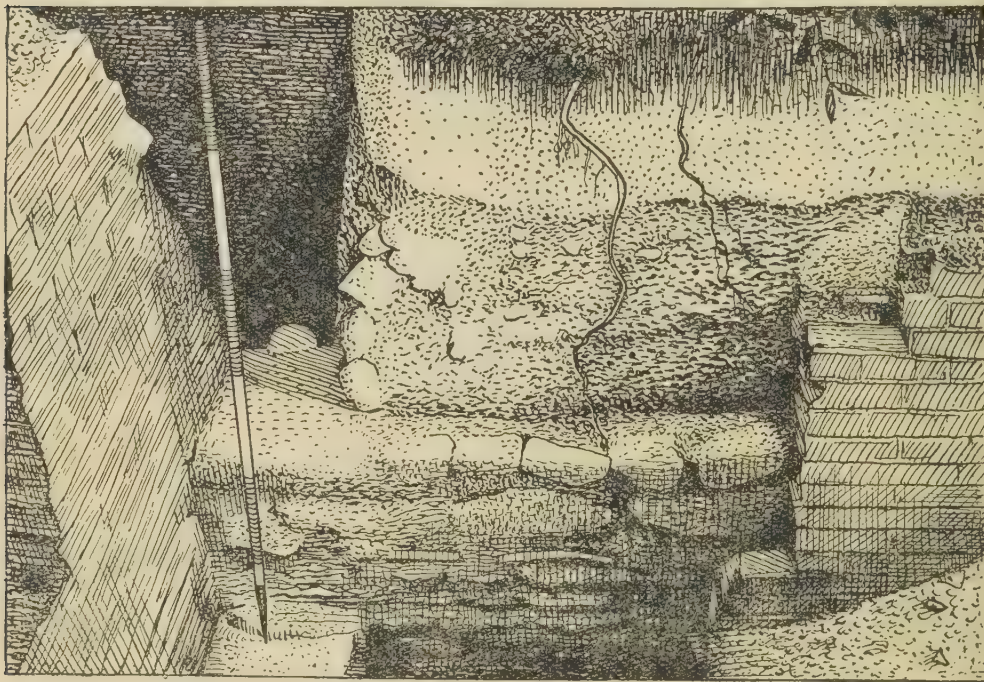


FIG 4. SOUTH WALL OF ST. GEORGE'S STREET BATH HOUSE MASKED BY MODERN BRICK WALL IN REAR PART OF CELLAR D; LOOKING SOUTHEAST

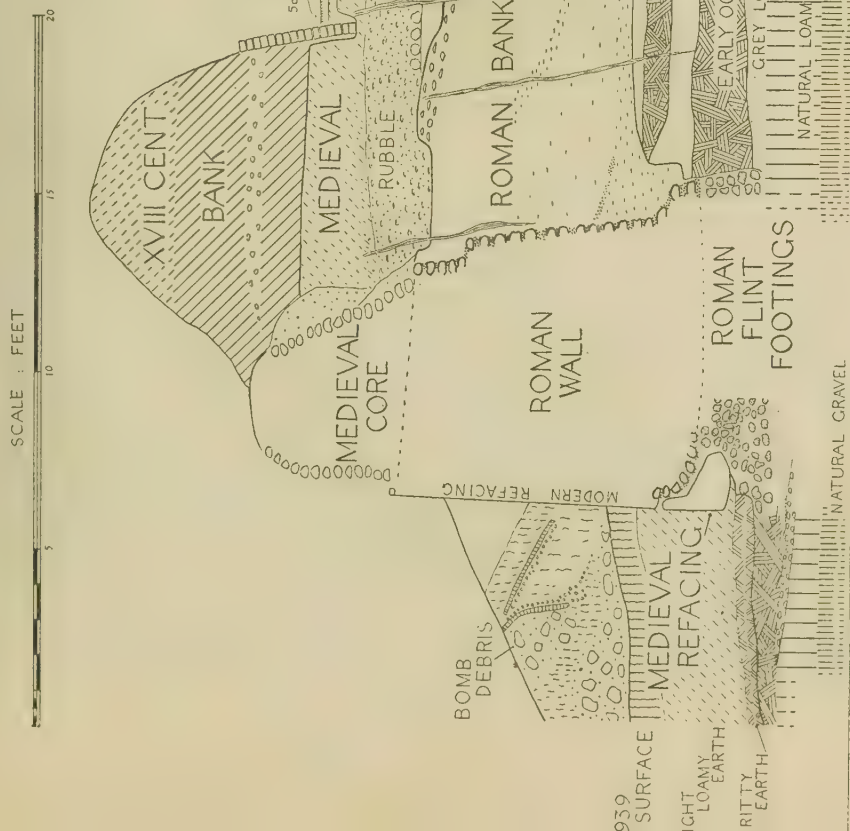
Drawn by B. Hope-Taylor from a photograph

sherd, though that of considerable importance. More recently two Saxon pits have been found, and a Saxon layer lying above the debris of one of the rooms in the S. Margaret's Street Baths. But on the whole it is true to say that Saxon levels are absent above the ruins of Roman buildings. Whether they exist in between the buildings remains to be seen, for little digging has been done except in cellars in such areas. There is convincing historical evidence for an extensive occupation within the walls certainly by the 9th century (3), and a straightforward reading of Bede (4) leaves little doubt that even in Ethelbert's time the nucleus of settlement was within the walls. If so, they must have avoided the gaunt ruins protruding here and there from piles of rubble; but if the

³ Birch, *Cartularium Saxonicum*, 515, 519, etc.; Stenton, *Anglo-Saxon England*, 519.

⁴ Bede, *Historia Ecclesiastica* 1, 25, 26.

CANTERBURY 1948
SECTION THROUGH DEFENCES
90 FEET SOUTH OF
RIDING GATE



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Roman town were never tightly packed with buildings, there would have been room enough for huts in intervening areas: and this perhaps explains why it was that the medieval streets lie over Roman houses.

THE DEFENCES

Until about a century and a half ago traces of three Roman gateways in the southern and eastern sections of the existing city wall attested the line of its Roman predecessor, and it was quite recently believed that the present wall itself was the work of the late 14th and 15th centuries (5). About the western sections of the Roman defences there was less certainty, but the presence of a chapel above the west gate in the reign of King Harold could be taken to imply a masonry gate on that site, and if masonry then doubtless Roman.

The present excavations have not yet dealt extensively with the problem of the defences, and the western part of the town is still unexamined. But in 1944 a section was cut just north of S. George's gate (6) revealing the Roman bank and the base of the medieval wall. The bank at this point was of clean loam six feet high, resting on natural soil. No dating evidence was found, but its absence could be taken to imply an early date. The medieval wall was dated to about A.D. 1300, a surprising result not reflected in the documents.

In 1948 a section was cut through the bank just south of the Riding Gate, and here again when the evidence is examined it will probably be found that the medieval wall was constructed about A.D. 1300. It may be that only the towers and gates were reconstructed at the later date indicated by the documents.

Unlike at S. George's Street, it was found here that the medieval wall had been built on top of a massive Roman wall of coursed flints and mortar, seven feet thick and still seven feet high (FIG. 5); and there was evidence that the medieval reconstruction was later than a small building on the floor of which lay several 13th or early 14th century jugs. This medieval floor rested on the summit of the Roman bank, a mound of predominantly cleanish loam capped with gravel. One is reminded of the cobbled rampart walk crowning the bank at York (7). The Roman bank itself contained much pottery, particularly in the dirtier tips, and this pottery is at earliest of mid-2nd century date; moreover the bank itself overlay earlier occupation layers, in one of which occurred a fragment of Castor ware. A date for this part of the Roman fortifications at the end of the 2nd century, possibly in the reign of Severus, seems provisionally probable.

This cutting threw much light on the Roman and medieval periods; but in truth it is a cross section of Canterbury's history in a fuller sense than that. Above the medieval refortification lay a late 18th century bank, constructed, as a neighbouring inscription testifies, in the years preceding 1802 and illustrating the rebirth of interest in the city's amenities at that time. Later still we have archaeological evidence of a great explosion sufficient to produce fissures in the Roman bank, and dated stratigraphically to the mid-20th century by deposits outside the wall, where twisted lumps of asphalt path and fallen wall lay above the pre-war humus; and this 'destruction-level' was succeeded by a period of calm before refortification, during which the acute angle between rubble and wall was silted up, and a turf-line formed. In the fullest sense a stratigraphical sequence from the earliest times to the present day!

⁵ *Arch. Cant.* XLV, 92-115, and D. Gardiner, *The Story of Canterbury Castle and the Dane John and its Manor*, 25-7.

⁶ By Mrs Audrey Williams, F.S.A., who was in charge of the excavations until January 1946. *Arch. Cant.* LIX, 69.

⁷ *J.R.S.*, xv, 177; XVIII, 67-8 and pl. VI, 2.

Roman Coins found in Iceland

by HAAKON SHETELIG

THE State Antiquary of Iceland, Kristján Eldjárn, M.A., published in January this year a fine volume containing a report of his recent excavations of pagan graves, and other contributions to early Icelandic history, under the title of *Gengidh á Reika*, Akureyri, 1948 (1). It is very important that we have got here reliable accounts of systematically explored Viking burials, with diagrams and photographs, all very well done, as Iceland had produced, till now, very little of similar publications (2). But really exciting is undeniably the news of the discovery of three Roman coins in Iceland.

The place of discovery was at the farm Bragdhavellir, at the head of Hamarsfjord, in the district of Sudhur Mulasýssel, on the southeast coast of Iceland. In a small valley called Djupibotn the gales had partially denuded the ground leaving only the hard stony gravel subsoil. In this place the remains of two primitive houses came to light, certainly representing an ancient farm which had been deserted for long ages. A peasant of the vicinity Jón Sigfusson started searching the site for antiquities and collected a lot of such poor objects as are generally left in country dwellings of the early Middle-Ages, nails and fragments of iron, broken pots of soapstone, stone whorls, some teeth of horse and cow, bits of charcoal, etc. The only object of a more distinct character was a bead of variegated glass, reddish-brown with black and white, possibly dating from the Viking Period. Subsequently a number of the antiquities were sent to the National Museum in Reykjavik, including two Roman coins said to have been found on the same site and under same conditions as the other articles.

One of the coins is an Antoninianus of Probus, found in 1905, and the other a similar coin of Aurelian found in 1933. The following year, 1934, the State Antiquary, Matthias Thórhásson visited the site and explored the ruins of the houses. He found a number of relics of the same description as already mentioned, but could obtain no further precise and definite information about the discovery of the coins. He was convinced, however, that there was no reason to suspect intentional fraud.

In the meantime a third Roman coin had been found in an equally surprising way. An English teacher, Mr Leonard Hawkes, was in Iceland in 1923, and visited the same neighbourhood in Sudhur Mulasýssel. In passing the Hvalsdal, between Hvalsness and Krossanes, he accidentally discovered a Roman coin in the sand. He searched a little with the fingers, but found nothing more. The coin is an Antoninianus of Diocletian and is now preserved in the National Museum in Reykjavik.

The three coins date from the time of the emperors Aurelian (270-5), Probus (276-82) and Diocletian (284-305)—a period some 600 years before the Norse colonization of Iceland towards the end of the 9th century, and thus is raised a most interesting problem concerning the early navigation of the north Atlantic Ocean. The first question

¹ The title means roughly : Go on the shore of drift, i.e. to gather drift timber, or drift wood.

² Daniel Bruun og Finnur Jonsson, Dálvikfundet, *Aarbøger for nordisk Oldkyndighed og Historie*, Kjøbenhavn 1910, p. 62. Daniel Bruun, Udgravninger paa Island, *Geografisk Tidsskrift*, vol. 17, p. 13. Kjøbenhavn. cf. *Viking*, vol. 1, Oslo 1937, p. 205.

to be answered is, then, whether the coins were brought to Iceland at the time when they were still the normal currency of the day, or at some later period. Matthias Thórdharon has expressed the opinion, after his examination of the locality, that the coins are not likely to have been left there in modern times, and should more probably be ascribed to visits of Celtic hermits or to the establishment of the first Norse colonists. In both cases the coins would have been first found as deposits in the earth in England and then brought to Iceland. The theory is not convincing, as the small copper coins had lost all appreciable value when they ceased to be accepted as current money (3).

Kristján Eldjárn rightly emphasize in the first place the fact that Roman copper coins are extremely rare outside the frontiers of the Empire, and adds the important observation that the three coins from Iceland are practically contemporary within a space of 30 years, of equal standard, and consequently represent the normal current money of the day in Roman territories. From this argument he concludes that the coins were brought to Iceland as the ordinary small change of Roman provincials, not as antiquarian curiosities from a past age. He points to contemporary Roman history as offering ample possibilities of explaining an accidental discovery of Iceland, e.g. the sea-power of Carausius as emperor of Britain, his fleets chasing Saxon pirates all round the coasts, and thus protecting also the commercial navigation in British waters.

Mr T. C. Lethbridge has kindly sent me his comments upon the coins from Iceland, recalling that there appears to have been serious trouble with the Picts about 306 followed by vigorous retaliation. During these several operations we might well imagine a Roman vessel storm-driven towards northwest as far as Iceland. The locality of the find on the southeast coast is perfectly consistent with this suggestion, which moreover provides a natural explanation of the find itself, and is thus stated by Lethbridge. I quote from his letter of 14 March.

'Had we however found a little collection of copper coins like the Icelandic find over here, it would certainly have been suggested some little votive offering made by a provincial Roman to a local genius. Several such little finds have been made in this district (of Cambridge). It does strike me as possible that the Iceland find might be compared with the kind of thing the old whalers used to do on the coasts of the Arctic. It seems to me quite possible that those Icelandic coins might have been put there by a Roman seaman who had been driven into the fjord in bad weather and who wanted to make some little offering, either as thanks for his escape or to ensure his safe return'.

If this theory be accepted, it supposes only a short visit on shore and satisfactorily explains why Matthias Thórdharon found no other Roman antiquities or antiquities of equal age during his investigation of the site. The lack of such associations was his chief argument for referring the deposit of the coins to the same date as the relics of habitation during the Viking period. But an occasional votive offering may well have taken place without leaving other traces of even temporary settlement.

The possibility of a Roman discovery of Iceland presents a highly interesting contribution to the early history of North Atlantic navigation. According to the Icelandic tradition the island was discovered about A.D. 850 by a Norse Viking sailing from The Faeroes to Norway and storm-driven towards northwest. He went on shore and stated that the land appeared to be uninhabited. Some 20 years later began the Norse occupation by emigrants from Norway, with considerable contingents from the Viking settlements in Scotland and Ireland. But the colonists soon discovered that they were not the first settlers in the Country, as they found in several places traces of primitive

³ Einar Ol. Sveinsson, *Landnám i Skaptafellstingi*. Reykjavik 1948, p. 2.

ROMAN COINS FOUND IN ICELAND

dwelling with crosiers, bells and books left by Celtic monks and hermits. The Icelandic tradition is testified by many place-names, such as Papey (the priests' island), still extant, and by the Irish legendary literature. It is sufficient to recall the histories of St. Brendan and St. Albeus of the 6th century, the monk Kormac of the 7th century, and many others, and the testimony of Dicuil about 825, the first exact geographical mention with reference to first hand contemporary witnesses.

Mr T. C. Lethbridge, in his recent book, *Merlin's Island*, has adduced strong arguments for regarding the Norse discovery of Iceland as inspired by their Celtic precursors. Iceland was well known among the Celts in Scotland and Ireland at the time of the Norse occupation, and many of the emigrants started from the Viking colonies in Celtic surroundings. It is more than probable that they had here got some notions of the Celtic voyages to the distant island in the Arctic Ocean (4).

The Roman coins from Bragdhavellir now raise the question whether the Celts were really the first discoverers of Iceland, or if they might perhaps have been acquainted with ancient reports, once current among seafarers of Roman Britain, of the fantastic island, covered with ice and with flaming mountains, far away in the northern mists of the Ocean.

⁴ T. C. Lethbridge, *Merlin's Island, Essays on Britain in the Dark Ages*. London 1948.

Einar Ol. Sveinsson, *Landnám i Skaftafellstingi*, has given a complete and excellent account of Icelandic place-names composite of 'papa', and a convenient review of Irish sources, as an introduction to his analysis of the Icelandic tradition of discovery and settlement.

Important New Books and Articles

The inclusion of a book in this list does not preclude its subsequent review

- SOIERIES PERSANES, by GASTON WIET. Vol. 52, Mémoires de l'Institut de l'Égypte, Cairo, 1947. 251 pages, 24 heliogravures. [Chiefly about the textiles from Bibi Shahr Banu near Raiy (Rhages), with 16 new fragments and a golden ewer from the same site (10-11 c.). Good bibliography].
- CHATEAUX TURCS DU BOSPHORE, by ALBERT GABRIEL. Mem. Inst. Arch. Stamboul, vi. Boccard, Paris, 1943. [Review in AJA, July-Sept. 1948, 468-9].
- THE ITALIAN ELEMENT IN LATE ROMAN AND EARLY MEDIEVAL ARCHITECTURE, by J. B. WARD PERKINS: annual Italian lecture of the British Academy, 1947. [Proc. Brit. Acad., Geoffrey Cumberlege, Amen House, London, E.C.4: sold separately at 7s 6d net].
- ON SOME ROMANO-BRITISH PLACE-NAMES, by KENNETH JACKSON. *Journ. Roman Studies*, xxxviii, 1948, 54-8. [At last the origin of these names is being discussed by one who has mastered the language to which they belong].
- THE JOURNEYS OF CELIA FIENNES (1685-1703). Cresset Press, 1949. 9s 6d. [The diary of an intrepid lady who rode on horseback throughout the country, penetrating as far as southern Scotland, and recording many acute observations, some archaeological, some about food, wine and people].
- WSTEP DO STUDIOW NAD OSADNICTWEM WIELKOPOLSKI WOZESNO-HISTORYCZNEY: by WITOLD HENSEL. Tom. v, Biblioteka Prehistoryczna. Poznan, 1948. 224 pages, 77 text-figures. In Polish, without résumé. [About so-called prehistoric but actually medieval, fortifications, especially Slavonic ones, in Bohemia and Poland. Contains publication of much new material hitherto inaccessible on account of the war].
- DIE SCHWEIZ IN ROMISCHER-ZEIT, by FELIX STAEHELIN. 3rd edn. Benno Schwabe, Basel, 1948. 659 pages, 205 text-illus., 1 map, 3 folding plans. 30 *Swiss francs*. [Revised and enlarged edition of the standard work on Roman Switzerland, combining archaeology and history, and listing every Roman find—a unique achievement].
- DIE TEXTILIENSAMMLUNG FRITZ IKLÉ-HUBER IM MUSEUM FÜR VOLKERKUNDE UND SCHWEIZERISCHEN MUSEUM FÜR VOLSKUNDE, BASEL; by KRISTIN and ALFRED BÜHLER-OPPENHEIM. Vol. 78, Mem. 2, Denkschriften der Schweizer Naturforschenden Gesellschaft. Published by Fretz, Zürich, 1948. 267 pages, 159 illus., 3 coloured plates. [An analytical description of textile technique, with good diagrams; the ethnological and archaeological specimens in this famous collection provide the means of discussing a very large number of types. Good bibliography].
- DER LINDENHOF IN ZÜRICH, by EMIL VOGT. Orell Füssli, Zürich, 1948. 231 pages, 59 text-figures, 44 plates, 3 folding maps. 30 *Swiss francs*. [Account of excavations, 1937-8. Important also for the technique of excavation in a built-up area. An early Roman military settlement of pre-Haltern and pre-Oberaden period, with pottery. Late Roman fort of 4th c. Pre-Carolingian buildings. Carolingian Pfalz of 9th c. Pfalz of 10th c., and fortified Pfalz of 11th-12th c. Many reconstructions of buildings: very detailed report on finds].

Reviews

BRITISH ART AND THE MEDITERRANEAN. By F. SAXL and R. WITTKOWER.
Geoffrey Cumberlege, Oxford University Press, 1948. £3 3s.

This is an expensive scrap-book, where the prehistorian will find the old friends of his youth—the Folkton drums, Aylesford bucket, and Bath Medusa—doing their stuff once more. But he will feel some exasperation at meeting them again, for the book adds nothing to existing knowledge. It claims to be 'a survey of the relation between the artists of Mediterranean countries and those of a country [Britain] beyond the Alps, from prehistoric times to the present day'. To do this adequately would require a range of knowledge beyond the attainment of the compilers. The help of specialists is acknowledged, and was obviously necessary. The term 'artist' is widely interpreted to include the makers of idols, town-planners and the builders of St. Paul's Cathedral. We suppose this may be legitimate, but art thus studied in a vacuum, divorced from the society in which it flourished, loses most of its meaning. Surely however it is stretching the connotation too far to include a row of portraits of Roman emperors just because they 'are known to have lived in, or passed through, Londinium'. Imperial progresses are seldom the source of artistic inspiration; and even if they were, these pictures would not show it.

The compilers would have done a real service if, instead of republishing old and familiar material, they had made and published photographs of objects still not at all, or inadequately, illustrated, such as the Marlborough bucket—a first-rate example of Late Celtic art. No scale is given for the illustrations, or for most of the plans, nor are there any indications of the sources, or references to published accounts; but many are already so familiar that this is unnecessary. But then, why publish them at all?

A map on plate xv purports to show the 'distribution of stone churches' ('after S. Pfeilstucker'), but omits one of the finest examples, that at Escombe near Bishop Auckland. The Roman theatre at Verulamium is illustrated (on plate xi) by an air-photograph so atrociously bad that at first sight we did not recognize it as such at all. Next to it is placed a photograph of the Albert Hall, whose inspiration is also Mediterranean. At the bottom of the page are Roman and 17th century English coins with the figure of Britannia. These illustrations show how in 1672 and 1871 British artists were harking back to Rome. The other three illustrations on this plate show how the alphabet of Trajan influenced the builders of Jarrow in A.D. 685 and Eric Gill a few years ago. The same alphabet, we might add, formed the basis of the lettering designed for the 5th (current) edition of the Ordnance Survey one-inch map, the models being photographs of actual inscriptions. If it were not for two bad photographs this would be an interesting plate, at any rate for the non-specialist, if he doesn't know the facts already.

Scotland has few direct connexions with Mediterranean art, but its one certain link—the vine scroll—is ignored. Many of the dates assigned are controversial. The proof-reading does not seem to have been properly done, to judge by the list of errata which itself contains an erratum—the emperor's name was Antoninus, not Antonius or Antonininus!

The preface contains a long list of those whose help is acknowledged. All of them are persons of distinction in their respective spheres, but we question whether their time might not have been better employed. Their names are not, however, attached to the sections concerned, and we are therefore left in some doubt about the degree of their responsibility. As we write, we have received a request to supply two photographs of Stonehenge for what would seem to be yet another book of a similar kind to be published abroad. The busy man is apt to regard the compilers of such books as an unmitigated nuisance.

In making an effort to supplement what has been said above by constructive criticism, one risks being told that a book should be criticized for what it is, not for failing to do what it does not aspire to. That would be a perfectly legitimate retort ; but as the chief criticism we make is that this is not the sort of book which is most needed by students, it would seem desirable for us to give examples of books which are so needed. There are many such, but amongst them would certainly be a descriptive catalogue of all British inscriptions of the Dark Ages, with really good photographs of each, taken in the right light by an expert photographer. Very few such photographs exist, and the taking of them would involve field-work. But field-work helps the student more than a pair of scissors and a paste-pot, and is itself an enjoyable occupation. Difficult subjects, such as are some of the memorial-stones, are a challenge to the photographer, who has the artist's thrill of satisfaction when he secures a good picture.

Another book could be devoted to the fine lettering of the tombstones of the 17th and 18th centuries. Scotland is particularly rich in examples of such ; the deserted graveyards of the lowlands must contain thousands, practically all of which are perishing unrecorded. An illustrated book would not only be attractive in itself, because of the beauty of the objects themselves, and of some historical value as well, but would also call public attention to a neglected and vanishing group of monuments. We are not aware of any book devoted to this subject, and of only a few articles, all indifferently illustrated.

The above suggestions assume that the publishers of a great University are fired with enthusiasm for the advancement of learning and willing to use their great resources to publish research which otherwise would remain unpublished ; for the research worker is seldom able to pay for the publication of his work himself. But is this assumption valid ? This book is, of course, published by Mr Geoffrey Cumberlege for the Oxford University Press, and not by the Oxford University Press itself ; but the distinction is one that laymen can never quite grasp. O.G.S.C.

EARLY INDUS CIVILIZATIONS. By ERNEST MACKAY. *2nd edition revised and enlarged by DOROTHY MACKAY.* London : Luzac & Co., 1948. 15s.

This book, under the title of 'The Indus Civilization', was first issued in 1935 and has been out of print for some years. It is the best available summary of the remarkable Bronze Age civilization first identified in 1921 at Harappā (Punjab) but better known from the well-preserved remains subsequently uncovered at Mohenjo-daro (Sind), and its re-issue is more than an act of piety on the part of its editor, Mrs Mackay. The editorial additions are of no great extent ; the main bulk of the work is as Mackay left it, and constitutes a useful, plain account of the remains then known, without any serious attempt at sociological interpretation. This omission might with advantage, be supplied in a future edition, since the material is now as abundant as it is likely to be for some years to come, and a careful assessment of its significance would be timely.

REVIEWS

In a purely constructive spirit, other suggestions may be offered for consideration in the preparation of the next edition. The revised title of the book, in its substitution of 'Civilizations' for Mackay's 'Civilization', is a misnomer. Only one Indus civilization is at present known—that which the editor rightly names specifically from the primary site, Harappā. Numerous other sites, mostly small, in Baluchistan and the Indus valley have produced heterogeneous industries, occasionally amounting to cultures but never attaining the status of 'civilization'. The confusion is perpetuated in the map facing page 1, where six 'Harappā' sites are associated without differentiation with five non-Harappā sites representing a variety of little-known industries and cultures. Incidentally, since no fewer than thirty-seven 'Harappā' sites have already been recorded and mapped elsewhere, the selection of a meagre half-dozen for the present map gives a very inadequate picture of the present state of knowledge. One of the more striking features of the Harappā civilization is its wide distribution, coupled with the outstanding size of Harappā and Mohenjo-daro, situated respectively in the northern and southern zones of intensity. These and other geographical factors invite discussion in the missing chapter on sociology, associated with a drastically revised map.

The greater part of the book deals with Mohenjo-daro, where Mackay himself dug for six seasons. A few references to Chanhudaro, the small but important site subsequently explored for a single season by the same excavator, have been inserted by the editor, without however giving any adequate indication of the evidence which it produced. Furthermore, in tabulating the culture-sequence of the site, the editor retains the illogical procedure of numbering from the top instead of from the bottom, so that her 'Harappā I' is later than her 'Harappā III'. A more logical sequence has already been proposed by Professor Stuart Piggott (*Ancient India*, I, 1946, p. 13) and is now used by students of the subject.

The type-site, Harappā, is very insufficiently described. A short and unintelligible account of the celebrated granaries there is concealed on p. 45, but no attempt is made to describe the remarkable and significant lay-out of the quarter in which these granaries are situated. I may perhaps be permitted also to regret that the massive citadel-fortification which I discovered in 1944 at Harappā, though at present unique and of obvious importance in the sociological assessment of the civilization as a whole, is not described or adequately discussed. Once more we miss the qualities of the constructive historical imagination which alone can bring these dead cities to life and give them meaning.

The section dealing with burial (pp. 73-4) is also obscure and incomplete. No mention is made of the fact that since 1937 a considerable part of a large Harappā inhumation-cemetery (R.37) has been uncovered at the type-site, clearly indicating the normal burial-rite of the civilization; and the omission is the more remarkable since one of the burials from this cemetery is in fact described and curiously noted as 'isolated'. Further, the reference to the well-known alien 'Cemetery H' at Harappa is so inadequate as to be meaningless. In all the circumstances, the retention of Mackay's view that 'the Harappā people usually cremated their dead on the banks of the river and that the ashes were then thrown into the water' is a manifest oversight on the part of the editor.

The illustrations (except the map) are generally satisfactory, and are more numerous than in the first edition. The old air-photograph, republished on a small scale as the frontispiece, is, however, almost valueless and would better be omitted; and plates I (i) and v show how an excavation should *not* be conducted. In short, the third edition will require extensive re-editing, after which the little book should continue to render useful service for a long time to come.

R.E.M.W.

ANTIQUITY

SOME ANCIENT CITIES OF INDIA. By STUART PIGGOTT. *Oxford University Press*, 1945. 5s.

This admirable little book was written in 1944-5, primarily for the many military and air-force visitors then in India. But in spite of its limited scope and purpose it has a permanent value as a fresh and well-written account of ten important sites, with a concise historical introduction and a chapter on prehistoric western India. This chapter deals mainly with the Indus or Harappā civilization, and in its broad lines might serve as a model for the chapter of which I have regretted the absence in my note on Mackay's book (above). Incidentally the air-photograph which illustrates it presents an intelligible picture of a part of Mohenjo-daro, although reproduced with too coarse a screen.

The chapter on Taxila is a succinct summary of that important and complex site, and its illustrations include an unpublished plan of the most elaborate building known from the earliest city (the Bhira Mound)—a large courtyard structure with a pillared hall, dating approximately from the time of Alexander the Great. Chapters on Sanchi, Ajanta, Ellora and Abu are lively summaries of the rich architectural, sculptural and pictorial features of those Buddhist and Jaina sites, and a selection of the innumerable monuments of Delhi is briefly described. Mathura has given the author a more ample opportunity for new work, and the sketch-survey of the extensive town walls is an addition to knowledge. Daulatabad, in the Nizam's Dominions, has also yielded new material, and the sketch-plan of this remarkable series of 14th century defences is the first published. Finally, two chapters deal with the famous Moghul complex at Fatehpur Sikri and Agra, concluding with an intriguing collection of opinions on the aesthetic value of the Taj Mahal.

The book as a whole shows how much can be achieved even by a hurried visitor (such as Professor Piggott would admit himself to have been) provided that he possesses a trained and discerning eye and an expressive pen.

R.E.M.W.

THE ANNUAL OF THE AMERICAN SCHOOLS OF ORIENTAL RESEARCH.

Vol. XXIV for 1944-1945, *Published under the Jane Dows Nies Publication Fund*, 1947.

Seal Impressions of Nuzi, by EDITH PORADA, PH.D. pp. 138, plates LIV.

This volume contains a selection of more than one thousand of the impressions on the tablets found at Nuzi, most of them in the house of a certain Tehip-tilla. The majority of these impressions is attributable to four or five generations of one and the same family, well-to-do citizens who were living at Nuzi in the fifteenth century B.C. The population at that time was predominantly Hurrian and appears to have been ruled by the king of Mitanni. Most of the impressions reflect the 'popular' style of glyptic which was widely diffused in Mesopotamia, Syria and Palestine at that period. This very rich collection is therefore an invaluable landmark in the history of ancient glyptic, since it enables us to pin-point a large number of different cylinders to a precisely measured time-sequence. There are also some earlier impressions which go back to the Jamdat Nasr period. Miss Porada has made an elaborate and detailed study of the evidence and has examined the comparative literature very carefully. This volume will take its place as a standard reference work for this period of Mesopotamian glyptic; the text contains much instructive comment on a variety of problems, many of which are not yet fully solved.

M. E. L. MALLOWAN.